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INSTITUTED 1832.

NEW SERIES.—VOL. I.

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TO MEMBERS.

*In issuing the present volume of "*Transactions*" of the "*Provincial Medical and Surgical Association,*" the first of a new series, the occasion may be taken of pointing out to the Members the importance of imparting to the Association the utmost efficiency of which it is capable. This is an object in the highest degree desirable at the present time, not only for the attainment of those purposes for which the Association was originally constituted, and for making its publications more worthy of support, but also for the keeping up of a bond of union among provincial practitioners, whose interests without some such Institution, by which their efforts may be at all times readily and powerfully combined, will be likely to suffer.*

It is earnestly hoped, therefore, that Members will individually exert themselves to induce such of their friends as may not hitherto have joined the Association to do so without delay, and to assist, by every means in their power, in the carrying out of its objects.

CHARLES HASTINGS,
President of the Council.

ROBERT J. N. STREETEN,
Secretary.

THE RETROSPECTIVE ADDRESS,

BY CHARLES COWAN, M.D., E. & P.,

Physician to the Royal Berkshire Hospital and Reading Dispensary;

DELIVERED AT

THE TWELFTH ANNIVERSARY MEETING

OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION,

HELD AT NORTHAMPTON,

ON WEDNESDAY AND THURSDAY, AUGUST 7TH & 8TH, 1844.

I shall not unnecessarily trespass upon the time of the Association, either by dilating upon the difficulty and magnitude of the task which has been assigned to me, or by contrasting my inferior qualifications for the undertaking with those of my esteemed and distinguished predecessors. Neither is it my intention to multiply apologies for numberless imperfections and short-comings, of which no one is more sensible than the writer; nor to weary you by pleadings for indulgence and sympathy, acting as I do in compliance with your choice and solicitation, and conscious as I am of having accomplished the duty entrusted to me to the best of my ability. No man can be fairly taxed beyond the amount of his capacity; and that humility is assumed, and those regrets are fruitless, which are founded upon the absence of powers we are not privileged to possess, and on the existence of defects we are unable to rectify.

The present Address, as the Members are probably aware, is more limited in its intention than were those which have preceded, the Council having decided, and I think most judiciously, to entrust

certain sections only of the annual retrospect to any one individual, for the purpose of lightening an almost unreasonable burden, as well as to facilitate greater fulness and accuracy of detail.

In consequence of this arrangement, the principal duty allotted to me on the present occasion is to glance at the history of practical medicine during the past year, which, if not involving an equal amount of scientific novelty with some other branches of our art, is yet prior to all others in its claim to our interest and attention.

Before, however, proceeding to the immediate business of the Address, I shall venture to solicit the indulgence of the Members to a few preliminary reflections.

When we take an extended view of the progress of medicine, tracing it from its scanty sources in the most remote periods of society, and observe its course as gradually augmented by the stores of Grecian and Roman learning, obscured by the darkness of the middle ages, and again bursting forth in the copious and almost overwhelming streams of modern literature,* we cannot but be astonished at the rapid changes it has undergone, and at the development which, in our own times, it has almost suddenly acquired.

Medicine, beyond all other branches of human knowledge, embodies the feeling and expression of the age. Embracing in its comprehensive range not only the learning and the science, but the ignorance of the day—modified in its theory and applications by every fluctuation in intellectual enlightenment—including among its votaries not merely a numerous body of eager investigators, but minds of every variety of power and attainment—in contact with every grade of society, and exposed to the invasion of every opinion and prejudice—it necessarily becomes impressed with the improvements, as well as tinged with the imperfections and peculiarities of the period in which it flourishes.

That such should be the fact is only consonant with the very nature of the human mind, which, either individually or collectively, seems never susceptible of more than partial activity. Each epoch of man's progress is characterised by the temporary prominence of particular truths, neglected or less clearly developed by former observers; and in the process of their establishment, the invariable tendency has been to invest them with exaggerated or exclusive importance.

* Bostock.

In proportion as knowledge is inaccurate are theories influential, supplying to the mind the felt deficiency of facts, and by their seductive completeness, proving adverse to patient observation. To guess and to imagine are at all times more easy than to test and to observe; and to extract a few scattered elements from the exhaustless storehouse of Nature, and weave them into fancied perfection by materials or rather notions derived from within, has proved, and still proves, a more attractive and flattering operation than to toil and labour in the accumulation of facts, conscious that our success must, after all, be most remote from our desires, and that, at best, we can only hope to add our mite to that temple of imperishable truth, which the best and soundest minds of every age have been slowly and toilfully erecting.

It is not difficult to perceive why abstract sciences have always preceded those depending for their progress on correct and patient observation—the former being founded upon the workings and constitution of the mind; the latter originating in acquaintance with the external world.

Man must, as it were, go out of himself in their pursuit and cultivation; he must possess the spirit of a child, be fully conscious of his ignorance, feel at every step his incompetency, be imbued with the love as well as the desire of instruction, and sit at Nature's feet the earnest but humble student.

Such are, however, not the feelings and promptings of youth; and long ages of chastening vicissitude must elapse before man practically acknowledges the true limits of his powers, and learns to subjugate his reasonings to the evidence of a rigid experience.

The science of medicine may be said to be now emerging out of infancy into manhood, to be gathering up the copious generalisations of the past and submitting them to the ordeal of inductive examination, to be testing hypothetical assertion in the stern crucible of experiment and observation, and to be giving to language an accuracy and a limitation of which before it was insusceptible.

We are, in fact, engaged in reducing an art into a science, and are arrived at the period of maximum difficulty and toil. The elements in our hands are fashioning and shaping for future forms; but though we can vision the dim outline of a higher and nobler erection, there is yet too much movement and bustle for accurate measurement of what we are accomplishing; things new and old

are strangely commingled, and impediments arise from the very number and zeal of the artificers.

There never was a period of such sudden excitement, such extensive and simultaneous activity. The unexampled development of the contributory sciences has furnished fresh and unexpected instrumentality for pathological inquiry, and compelled renewed investigation in accordance with the nature of the means now in our possession.

Chemistry, in all its protean and still unfolding applications, is closely approximating to the phenomena of life, and becoming daily more and more available for the detection and treatment of disease.

After having in a few brief years almost exhausted our knowledge of healthy and morbid structure, and reduced this important branch of inquiry into order and arrangement, the narrow limits of unaided sense are again marvellously enlarged by microscopical discovery, our past acquisitions regarded as elementary and incomplete, and the whole subject is again to undergo renewed and unexpected revision.

From similar causes the fluids as well as the solids are attracting fresh interest and examination, and from their careful analysis we are daily being furnished with important diagnostic and therapeutic indications. Indeed, wherever we direct our gaze, everything appears advancing and unsettled; we seem engaged rather in the eager pursuit, than to be in the enjoyment or possession of truth.

Another most striking feature of our present condition is the unexampled facility afforded by the press for the expression and diffusion of thought. Every idea now finds an easy channel for utterance; in our periodicals and other publications encouragement is given for the most fleeting expression of opinion; and investigations, whether trivial or important, are equally honoured by publicity. The necessary consequence has been, that evil has been mingled with the good; our literature has become most inconvenient from its bulk, full of crudities and endless repetitions, heterogeneous and fragmentary in character. The student is wearied and confused by the very extent of the surface, the exercise of thought is impeded by so continued a draught upon his powers of acquisition, and what is original and vigorous in the mind is too often repressed or entombed.

The natural tendency upon an ambitious spirit, if not of the highest and most comprehensive order, is to over-stimulate and exhaust its energies in the mere effort to acquire, to consume beyond the power of digestion, and either to rest with slavish dependence upon the dogmas of others, or, escaping from what is felt impossible to reconcile, to pursue the path of ignorant self-reliance, reckless of all authority and experience.

We are kept in a constant state of expectancy and interrogation; doubt pervades everything, and truths which ages have confirmed are exposed to assailants of every form and dimension, boldly requestioned and challenged by the experimenter of a day. Our periodical literature and more formal treatises, inconveniently and undesirably multiplied, teem with detail and assertions of the most superficial and contradictory character; yet all, as in olden times, based upon *facts*, and raised to a higher nominal authority, because supposed to be founded upon a purer inductive philosophy. We are inundated with dissertations on the proper methods of study, and every writer, even the smallest, vaunts the strictness of his obedience to Baconian injunctions. Philosophers have multiplied faster than philosophy itself; and if we practised as well as we preach, our successors might anticipate a period of professional elysium.

While making these remarks we are not overlooking the far more gratifying and unquestionable consideration, that medicine, both as a science and an art, is really advancing; that the labours of a few master spirits, particularly during the past half century, have astonishingly extended our knowledge of the nature and seat of disease; that we are in possession of a discriminating power, in diagnosis and treatment, to which our predecessors were strangers; and that, provided as we are with new instruments of research, and furnished with additional therapeutic resources, we may yet calculate upon much increase beyond what has already been attained. But still, it is no less true that we have our disadvantages, and there are clouds which bedim the horizon of our future prospects.

We are too apt to forget that the great work we are now engaged in is rather that of explanation or development, than of discovery; the clearer illustration of what is old more than the revealing of what is new; reducing into accuracy of classification and expression what before was only general and vague, and demonstrating the

soundness, not the absurdity, of many of the ideas and conclusions of our predecessors.

We must not estimate our present progress by mere numbers, opportunity, or zeal, nor suppose that the multitude will ever be directly instrumental in the extrication of truth: no, this holy and glorious mission will ever be the privilege of the gifted and laborious few, and its accomplishment be impeded rather than advanced by the undue multiplication of labourers.

However excellent the system we adopt, however perfect the scheme of our inquiries, the true value of results must ever chiefly depend upon the power and qualifications of the observer. Few, very few, possess these to the requisite extent; and hence we are daily overwhelmed with materials too incomplete and amorphous ever to become the foundation of scientific induction.

The *numerical method* has been loudly proclaimed as the only cure for our discrepancies, and by many has been eagerly hailed as the harbinger of future agreement; but, regarded in its applications to clinical medicine, its powers are limited, and it clearly possesses no inherent exemption from fallacy, the correctness of figures depending on the similarity and accuracy of the facts they represent; and the latter are constantly varying in their value with the character and opportunities of the observer.

Much useless discussion would have been spared had considerations, so self-evident, been properly estimated; and we should have moderated our expectations of the efficacy of an indispensable instrument of scientific research, had the real difficulties attending its employment, under the circumstances we are alluding to, been sufficiently weighed. The defect resides not in the method, but in the very nature and infinite complexities of bedside investigations, too varying and fluctuating, we believe, ever to be susceptible of accurate numerical expression. At all events, before this can be the case, the natural history of disease must be clearly traced, the effects of medicine ascertained, and the variations induced by season, age, individual peculiarities, and the almost numberless disturbing influences from within and from without, be capable of clear appreciation. And how, when, and by whom, we would ask, is all this to be attained? The problem appears to us too vast and too complicated ever to admit of an absolute solution. Science will never neutralize the value of individual sagacity, nor

induce unanimity of practice or opinion by means of arithmetical rules.

Though strongly advocating the utmost possible extension of numerical research, substituting as it does the accurate for what is vague, and regarding its varied and extensive applications at the present moment as among the distinctive peculiarities of our times, we yet feel that in the practice of medicine its uses are limited, and that there are evils as well as benefits resulting from its undue application. Not only is it incapable of preserving us from mistakes as egregious as any which have been fostered under the less definite sanction of experience, but it imparts to falsehood a fearful semblance to truth, and gives to assertion an absoluteness and precision well calculated to satisfy the inquirer and repress the effort of independent observation.

We cannot doubt that future researches will furnish many singular refutations of arithmetical medicine; and it will be again recognised that the practice of our art must ever be based upon a few but comprehensive general principles, carried out more or less successfully into their individual applications as they are wielded by a mind of greater or less acquired and intuitive capacity.

Science will, doubtless, accomplish much by increasing our acquaintance with the details of disease, by facilitating and multiplying the means for its recognition and cure; but we cannot perceive that it has yet revealed a single leading principle not recognised by the soundest thinkers of ages which are past, nor has it yet enabled us to escape from a warfare of hypotheses as violent and contradictory as any which has characterized the darkest periods of our history.

At the present moment of mid-day enlightenment, as it is fondly termed, there is nothing too visionary or absurd for serious discussion or belief; and earnest disciples as we are of a purer inductive philosophy, and lofty expounders of former discrepancies, not only among the people, but with many members, alas, of the medical profession, the most irrational systems find their dupes as well as advocates, while the press teems with productions as extravagant in their assertion, as opposed to all soundness of observation and reasoning, as ever was the case in times we are now fond of contrasting.

It is true that modern lucubrations are invested with the outward garb, however hostile they may be to the spirit of a rational philosophy, and error as well as truth, under the more finished drapery of convenient phraseology, lays claim to similarity of system and proof; yet such is the singular contradiction, the strange and unnatural conjunction of good and evil in our actual condition, that while the past half century will ever be appealed to as the most fertile and important in the long records of our art, it will nevertheless be hereafter selected as furnishing, even from our own ranks, some of the most palpable evidences of human folly and delusion.

While we therefore cheerfully acknowledge that there is much to encourage us in the era at which we are arrived, much to gratify future anticipation of good, much on which we may rightly congratulate each other, it cannot be concealed there are, combined with our advantages, many counteracting causes of difficulty and distrust, many circumstances obstructive as well as aiding our advance, a consciousness of which should sober our exultation, and unite us more seriously together for their rectification and removal.

Cultivating, by association and intercourse, lofty principles of thought and action; disrobing the heart from low and selfish motives; restraining all inordinate ambition; being polarised, so to speak, in the direction of the general rather than of individual good; attaining clear views of the true limits of human intelligence, and according to revealed truth its rightful study and authority—these are the means on which we would chiefly rely for achieving that soundness and sobriety of mind, without which man's intellect riots in wild extravagances, and science only ministers to a restless and vagrant fancy.

Virtue and wisdom are natural allies to each other, and they have ever been most closely united in those to whom mankind is indebted for the most solid and lasting contributions. To cultivate the heart as well as the head is the object of our own and all kindred Associations; and there never was a time when such a combination was more to be desired for ultimate progress and safety. In proportion as our individual standard of moral excellence is high, will our collective energies be rightly directed, and the great end of our union most completely secured. But human

nature must be sinless before our philosophy is pure; the heart must be untrammelled before the intellect is free.

I am fully aware that differences of opinion will ever be entertained upon sentiments like those I have now ventured to express; but I am encouraged by the conviction that their spirit will be responded to by many I have now the honour of addressing, while they will meet from my opponents, if such should be present, a kind and indulgent consideration.

Apologising for the unexpected length of these introductory remarks, I will now proceed to a brief summary of the principal subjects of practical interest which have been presented to my notice during the past year.

EPIDEMICS.

I quite agree with Dr. John Gardiner, the philosophic and Christian author of the *Great Physician,* that no satisfactory hypothesis has yet been proposed to embrace the recorded phenomena of epidemic visitations. Comets, meteors, planetary conjunctions, earthquakes, volcanoes, atmospheric changes, telluric effluvia, parasitic animals, &c., have at various times been proposed to account for pestilences; but their progress, limits, and termination, are still inexplicable, and cannot be referred to any adequate and efficient *second* cause. This obscurity in their origin does not, however, justify remissness in their observation, or blind empiricism in their treatment; and there is much practical soundness in the observations of Professor Forget,* who insists eloquently upon the evils resulting from considering epidemic diseases as essentially different from those which are sporadic. He warmly advocates the opinion that, with few exceptions, they are ordinary affections, plus, increased generality and intensity, and urges the importance of treatment being regulated by the principles which guide us under other circumstances. Such a view would tend to give confidence in moments of terror and emergency both to the practitioner and patient, and prevent those random therapeutic experiments which have too often become just causes of ridicule and reproach to our art; it is also consonant with our recent experience in the instance of Asiatic cholera. Admit even a disease

* *Gazette Medicale de Strasbourg*, Juillet, 1843.

to be new, a rational treatment is at all times the most likely to be successful.

It has been ingeniously suggested that the epidemic cause, modifying, as is now well ascertained, the system of healthy individuals, is, to a certain extent, a *protection* against human infection; and hence the reason why in places free from the *epidemic* the disease is most easily communicated from person to person.

Dr. Klencke, of Brunswick, is pursuing a very interesting series of experiments on the subject of contagion. He appears to have succeeded in transplanting the contagious principle from one animal to another, by means of the cells of carcinoma, tubercle, melanosis, condyloma, and warts; mucous membranes affected with ozæna and coryza; carbuncle; hydrophobia; the vaccine virus, small-pox, and the other acute exanthemata; and he concludes that contagious diseases are capable of transmission by means of cells, which enjoy a special and semi-independent vitality. The subject is one of peculiar interest and importance, and stands much in need of careful and consecutive investigation.*

Plague.—M. Aubert† has fortunately attempted to elucidate the ever litigated question of the contagion or non-contagion of the plague by a wiser and safer process than endeavouring to reconcile the conflicting opinions of preceding writers. He has diligently examined the registers of the Lazarettos of Marseilles and Malta, for the last hundred and twenty years, and patiently analysed all the recorded cases. The result of this most laborious undertaking has been the important conclusion, that not a *single* case has occurred during this long period in which the plague has *primitively* appeared on board, or in the Lazaretto, *eight days after* the departure of the vessel from the infected locality.

Another singular fact brought to light by M. Aubert's researches, is that no instance can be cited of any individual engaged in the Lazarettos suffering from the disease, although no kind of precaution is resorted to in order to prevent contagion. If the facts as stated in M. Aubert's memoir be correct—and his data are most convergent and extensive—all discussion as to the duration of the

* *Arch. für die Gesamte Med.*, from *Provincial Medical Journal*, Sept. 23, 1843.
† *Lancet*, March 23, 1844.

incubation of the plague must henceforth cease, and the laws of quarantine will at once have clear and intelligible limits. A recent and apparently exceptional fact has been warmly discussed in the French Academy; but the general opinion was strongly in favour of the value and correctness of M. Aubert's conclusions.

Fever.—I am indebted to Dr. Golding Bird* for the following allusion to Professor Mülder's very interesting researches on the oxides of protein. A few years ago it was discovered by Professor Mülder, that various animal elements, when dissolved in a dilute solution of potass, resulted in a fluid of identical composition and qualities. This body he named protein; and he has since ascertained that it combines with two or more quantities of oxygen in the blood. These changes are effected, according to Dr. Mülder, by the process of respiration, which, when accelerated, gives to the blood its buffy or inflammatory covering. Fever, he supposes to depend on the development of more oxyprotein than in health; and when a large excess of this matter is collected, all the phenomena of inflammation take place, and these compounds of protein are effused as inflammatory products.

Whilst Mülder agrees with Liebig in the opinion that an increased absorption of oxygen exists during fever and inflammation, he altogether differs from him as to the element which acts as the absorbing agent, believing that there is no evidence of the process being effected by the change of the iron of the red particles from a protoxyde to a sesquioxyde. He offers no speculation as to the cause of fever, which he simply regards as the pathological condition which induces the generation of the oxyprotein. Dr. Bird justly remarks, that like all chemical theories of health or disease, they should be received with caution, as a more melancholy error can scarcely be committed than that of explaining the phenomena of the animal organism too exclusively on chemical principles.

The sagacious and cautious Andral concludes, that in the actual state of our knowledge the character of fevers remains still a negative one; that is, the fever which accompanies the pyrexiæ does not acknowledge, either in the solids or in the blood, any alteration which can account for it—a conclusion not very flattering to human vanity after the infinite attempts which have been made to establish a more definite result.

* *Medical Gazette*, February 10, 1844.

Dr. Metcalfe, in his recent and most remarkable work on "Caloric," ascribes fever to a reduction of temperature, and considers all the varieties of fever as modifications of the same disease, arising from different degrees in the intensity or duration of the causes producing them—the extent to which the vital properties of the blood become impaired, determining either a quartan, intermittent, or the more deadly forms of typhus, yellow fever, &c.

The greater number of the sittings of the Belgian Academy during the past winter have been occupied by a most protracted and vehement discussion on the causes and pathology of fever;* and though the debate was conducted with great talent, and evinced no ordinary acumen and sound medical education on the part of the members, it terminated as it begun, each party retaining their opinions, and the whole evidencing the feebleness of man's powers to soar beyond the confines of simple observation. The Broussaists were in a decided minority, and the tendency to return to the humoralism of past ages characterised the statements of the greater number.

The importance of being acquainted with the natural history of disease, and the rate of mortality, apart from medical interference, is nowhere more evident than in fever. Dr. Burton, in an excellent paper on the statistics of fever in St. Thomas's Hospital,† observes, that the histories of the epidemic fevers which existed in early times, and the numerous records of their more recent occurrence in localities remote from one another, furnish sufficient support to the opinion of the probable continuance of an universal and variable, but constant state of mortality; and that the healing power of physic is controlled by causes, the fatal influence of which can never be entirely obviated by medical art. Few circumstances exert so great an influence as season, the deaths having been nearly double when the temperature was low; and for twenty-three years no material difference in the rate of mortality had occurred. It appears to be uniformly greater among male patients, and in an inverse proportion to the age.

For the important law above referred to we are indebted to Mr. A. Watt, City Statist for Glasgow,‡ who has recently compared his results, as obtained in England, with the tables in America, and from

* *Archives de la Medecine, Belge.*

† *Medical Gazette*, November 17, 1843.

‡ *Proceedings of the Philosophical Society of Glasgow*, January 3, 1844.

their strict accordance, feels justified in deducing that the amount of deaths by various diseases, such as fever, measles, scarlatina, small-pox, and hooping cough, is nearly identical at the same ages; and that whatever the total amount of deaths by each disease may be, the proportion which the deaths falling at certain periods of life bear to the whole deaths by these respective diseases, *remains the same*. This remarkable result points out the existence of yet unknown influences regulating the amount of deaths at different ages, and is also valuable for arriving at a true knowledge of the sanitary conditions of towns, and for determining the existence of excessive mortality. It need scarcely be suggested how directly such facts bear on the reputed value of certain methods of cure, and prove that the solution of a therapeutic problem, though often lightly attempted, is a task of no ordinary complication and difficulty.

Mr. Richard Hindle* has directed attention to the frequent occurrence of *mild shivering*, or rather a *coldness*, supervening after the seventh or eighth day of continued fever, but most frequently from the eleventh to the fourteenth days, and followed in all the cases he had observed by recovery. The fact has not escaped the observation of Hippocrates, but does not seem to have attracted the attention of modern observers. The true value of rigor is far from being practically settled; if often the earliest indication of local or general mischief, it may be equally significative of the decline or cessation of morbid action.

Mr. Morris, of Spalding,† in a brief but graphic description of the typhus which prevails in the marshy districts of Lincolnshire, states, that he has never known a relapse after complete convalescence, where the petechial eruption had existed; but under the opposite circumstances, relapses were frequent, and the patient even seemed additionally predisposed. The small amount of the eruption in typhus, and its evanescent character, render it difficult to attach to it the importance it would appear to deserve; the remark, however, is interesting, and seems to justify the approximation of typhus to the class of eruptive fevers. Mr. Morris speaks very decidedly as to its contagious character, and adverts to the rapidity of its diffusion in an atmosphere charged with malaria. He also states that the mortality was greater among

* *Provincial Medical Journal*, October 7, 1843.

† *Provincial Medical Journal*, January 6, 1844.

teetotallers than others, a fact warmly controverted by the zealous advocates of abstinence, but consonant with the known antiseptic and respiratory effects of alcohol upon the system. In the treatment of head symptoms, he speaks very highly of the effect of a drachm of tincture of henbane in camphor mixture, repeated every four hours, in allaying irritability and pain, and says that he has often observed fatal results follow antiphlogistic measures.

The value of calomel in typhoid fever has been extensively tested by Drs. Lombard and Fauconnet,* of Geneva, who thus sum up their elaborate experimental researches. Calomel diminishes the mortality in typhoid fever, and renders all its symptoms less severe, more especially those which indicate a disturbance of the nervous centres. It tends to abate the danger of any thoracic complications; it modifies and corrects in a very remarkable manner the condition of the alvine evacuations; it rapidly cleanses the tongue, and renders it and the mouth less parched; it dissipates tympanitic distention and colicky pains, and exerts a favourable influence on the intestinal complication. Dr. Dielt, of Vienna, in a memoir we shall shortly allude to, has arrived at very similar conclusions from extensive comparative trials. Procuring the mild influence of mercury upon the system, in all cases of continued fever, is advocated by many British practitioners, but the use of this mineral is certainly less appreciated on the continent generally than in England. That we often abuse as well use it, cannot on the other hand be denied.

The employment of purgatives in fever has met with an intelligent investigator in Mr. Hunt.† He very rightly considers many of the functional disturbances to have a remedial tendency, and objects to the routine interference with a torpid state of the bowels in the low stages of fever, regarding the condition as favourable to digestive and nutritive repair. He has known seven days to elapse without an alvine evacuation, and with the greatest apparent advantage, and we believe many practitioners have had to regret the exhibition of even the mildest laxative in the later stages of fever, accompanied with much abdominal irritation.

Endeavouring by alterative and laxative medicine to render the excretions healthy, must necessarily be fruitless, if not injurious, so

* *British and Foreign Medical Review*, January, 1844.

† *Provincial Medical and Surgical Journal*, May 8, 1844.

long as the entire system is specifically disturbed, and yet it is often attempted, as if the individual were otherwise healthy. To proscribe laxatives altogether, would be equally undesirable. There are many degrees between the systems of Hamilton and Broussais, but in England we are perhaps too much impressed with the necessity of regular alvine discharges under all circumstances, and sometimes forgetful of the peculiarities of diseased existence. The right use of purgatives in fever is unquestionably a point of great practical difficulty.

Dr. Searle, of Bath,* advocates the use of bark or quinine in all cases of fever, including rheumatism, erysipelas, and the eruptive diseases, after the early antiphlogistic measures, and narrates some interesting cases in support of his views. The remarkable contradiction in the experience of competent observers on the use of bark in fever, presents a striking illustration of the difficulties attending correct therapeutic inductions.

A practitioner in Ireland states,† that in thirty cases of continued fever he has obtained most gratifying results from the free use of ammonia. He dissolves a drachm of ammonia in eight ounces of cinnamon or plain water, adding three or four drachms of the spirit of nitric ether. Of this he gives at first two tablespoonfuls every second hour for five or six doses, or until some benefit results, and afterwards every third hour, more or less frequently according to circumstances. It is possible that the efficiency of such a remedy might prove great in some forms of fever, and negative, if not injurious, in others. How to discriminate, except by bedside experiments, we know not.

Another observer, Dr. Gruenbaum,‡ says that he has employed dilute phosphoric acid, and with great success; and if Liebig's views on the phosphatic salts are correct, this agent or its combinations may be deserving of more attentive consideration.

Dr. Francis Devay, of Lyons,§ in his clinical researches and observations on malignant fevers, gives a decided preference to musk, over every other stimulant. It acts most effectively, he says, in cases characterised by general nervous incoherence, and introduces into the system a principle of quiet and repose. He

* *Lancet*, July 13, 1844.

† *Medical Times*, January 15, 1844.

‡ *Casper's Wochenschrift*, and *Lancet*, September 15, 1843.

§ *Revue Medicale*, Juillet, 1843.

prescribes it in the form of pill, every hour or half-hour, often combining it with camphor, and insists on the importance of not waiting until the unfavourable symptoms are fully developed.

Few diseases have met with more competent and zealous describers than the epidemic remittent fever, which has extensively prevailed in Edinburgh and other large towns of Scotland, during the summer and autumn of 1843. In the admirable volume of Dr. M'Cormac, and in the essays of Drs. Craigie, Alison, Smith, Gibson, Kilgour, Reid, Arnot, Henderson, and others,* nothing has been left unsaid which can elucidate the origin, progress, symptoms, and treatment of this wide-spreading visitation. In England, Ireland, America, and parts of the continent, a less diffused but similar type of disease has partially prevailed.

We shall not attempt to follow the arguments which tend to prove that the disorder is identical with the yellow fever of hot climates, or with the well-known and well-described epidemics of Dublin, nor shall we discuss its analogy with or difference from ordinary typhus, nor whether it may not have been known to the father of Greek medicine, being less learned and less interested than perhaps we ought to avow, in such profound and recondite questions; but shall content ourselves on the present occasion with a brief summary of its leading peculiarities. The short duration of the fever; the rapidity and violence of the early symptoms; the almost constant crisis and false convalescence between the fifth and ninth days; the equally constant relapse about the fourteenth day; the absence of the true measly eruption of typhus, (with few exceptions,) though petechiæ were observed; the frequent yellowness of the skin; the obstinacy of the sickness and vomiting; the occurrence of critical sweatings, and the subsequent rheumatic pains, conjoined with its wide prevalence and low mortality, may be cited as the leading and characteristic features. I have met with nothing in the treatment of the complaint which calls for particular observation.

Professor Rossi, of Turin,† has examined the state of the blood during an epidemic in that city, similar to the one just alluded to. Patients were bled in every stage of the fever, and he also examined

* *Edinburgh Medical and Surgical Journal*, October, 1843; *Scottish and North of England Medical Gazette*, October 7, and December 2, 1843.

† *Medical Times*, February 17, 1844.

the blood obtained after death from the heart and vena portæ, contrasting his results with similar experiments upon healthy individuals. The principal point of interest is the fact, that in every case an inflammable gas was discovered, presenting all the characters of cyanogen. It will be recollected that Liebig considers cyanogen or some of its compounds as the cause of malaria.

Since January of the present year, typhus has reigned epidemically in Paris. It was at its maximum of intensity in April, had a strong tendency to a remittent or intermittent form, and appeared to increase with a westerly, and to diminish with an easterly wind. Erysipelas has also prevailed during the same period.

Dr. S. Forry's description* of a malignant fever prevalent at Roudout, New York, in August and September, 1843, closely resembles that of the Scottish epidemic.

The distinction of fevers on the ground of the predominance of particular symptoms, is endless and unphilosophical. Probably every epidemic is more or less special either in its circumstances or cause, and while the actual type of fever requires careful study when deducing the treatment, each change in the manifestations does not justify the assumption of a new disease. A difference in degree is no real difference in pathology, and experience has now shown that textural changes after death are constantly fluctuating in their nature or amount. Other influences apart from the exciting cause of an epidemic, materially modify its progress and results; and we are probably ascribing too much to a change in the disease, and too little to a change in external circumstances. It is on this principle we would explain the frequent variations in type which continued fever assumes, and the disappearance or return, after longer or shorter intervals, of diseases of a similar character; and hence also the probable reason of the discordant statements of writers on the subject, deriving their experience under very varying climateric and social conditions. We have never been able to partake of the enthusiasm excited by the discovery of intestinal follicular changes, and have often felt wearied at their minute and endless description, unable to grasp the immense importance of the fact, or to perceive its asserted bearings upon the explanation or pathology of fever. That it is desirable to know every change which disease induces in our organs, no one can reasonably deny, and in propor-

* *New York Journal of Medicine*, November, 1843.

tion as pathology is accurate is our treatment successful; but the tendency to over-estimate them has been strikingly displayed in modern times, and incalculable labour has been devoted to subjects of very secondary importance. Such views may be regarded as heretical by many, but of their ultimate prevalence and truth we feel confident.

We cannot conclude this section of our address, on continued fever, without adverting to the admirable description, by Dr. Dielt,* of the epidemic of typhus which ravaged the Austrian capital during the years 1842 and 1843 He seems a worthy successor of the Stolls and Franks and Hildebrands, and the disciple of a school which has never manifested that exclusive dogmatism, at once so opprobrious to true philosophy, so unfavourable to the formation of the rational practitioner. "During epidemics," Dr. Dielt justly remarks, "the superiority and nobleness of the medical art become apparent; the scientific physician then evinces the dignity of his calling; the friend of truth and of humanity, he stands forth, gloriously distinguished from the crowd of false and ignorant pretenders, who shrink during public calamity into their native and merited obscurity." While they often exhibit the powerlessness of medicine, epidemics never fail to illustrate the value of knowledge. We may not be permitted to cure, but we actively minister to successful results; and from the unity of type and multiplication of cases, rational experiments may go hand in hand with sound observation, and many questions of therapeutics be affirmatively or negatively determined.

Dr. Dielt noticed a singular contrast between redness of the tongue and the amount of abdominal pain. If the tongue were pale the abdomen was painful, and soon after tympanitic. A red tongue was the best indication of the favourable progress of the fever, a fact not in accordance with the phlogistic school.

Another interesting remark by the same observer is, that acute tuberculization of the lungs prevails immediately before and during an epidemic of typhus. He infers that tubercular deposit is provoked and rendered acute by typhoidal intoxication, and that the two diseases are often proceeding together in nervous, phthisically predisposed individuals. Typhus loses by this alliance its special intestinal characteristics, and pulmonary tubercles are not manifested

* *Archives de la Medecine Belge*, November and December, 1844.

by their ordinary diagnostic phenomena. This remark is interesting, because the antagonism of typhus and consumption has been strongly advocated. It is, however, singular that no case of chronic phthisis suffered from typhus, though the value of such a fact is rendered negative by the circumstance, that those enfeebled by anxiety or deficient food were very seldom affected. The epidemic influence attacking principally the adults and those the most healthy and robust, would alone account for the previously consumptive escaping. We confess ourselves quite at a loss to hazard any trustworthy solution of such apparent contrarieties.

In the estimate of treatment Dr. Dielt points out the essential importance of attention to the stage of the epidemic, at one period proving fatal in spite of all interference, at another the majority recovering under every variety of means. All co-existing diseases partake of the epidemic peculiarities, which has led to the grouping of ordinary diseases under the head of the epidemic, and thus leading to incorrect inferences. Dr. Dielt extensively tried the mineral and vegetable acids in every form and dose, and, though strongly prejudiced in their favour, was quite satisfied that, without exception, they aggravated the abdominal and general symptoms. We regret he made no comparative experiments upon the use of alkaline and neutral salts.* Quinine was injurious; chlorine appeared negative; and to the use of calomel alone, in doses of one-eighth to one-half of a grain, every two or four hours, could he ascribe any positively beneficial results.

A decided exacerbation of the epidemic was observed every third week, but in proportion as the cases were severe was their number diminished, as if the quantity of the poison remaining the same, its diffusion was subject to periodical contraction and expansion. This memoir is well worthy of perusal.

Malaria.—The nature and origin of malaria is as much as ever a debated question. Dr. Fergusson,† whose experience has been most extensive, supports the telluric theory, while Dr. M'c Cormac

* It would appear from some recent researches, that the state of the urine is no test for determining the use of either acids or alkalies in typhus fever. In the early stages it presents a strong acid reaction, while it subsequently becomes neutral and then alkaline; when the disease is beginning to decline, the acid reaction again sets in.

† *Edinburgh Medical and Surgical Journal*, July and October, 1843.

considers malaria to be exclusively the result of vegetable decomposition, aided by a temperature ranging from 80° and upwards.

The ingenious Raspail* admits both sources, and considers marsh effluvium to consist of a peculiar acid, which by poisoning the blood enfeebles and enervates the entire organism. Telluric emanations are favoured, he thinks, by currents of air acting as a vacuum over the surface of the earth, and thus disengaging the noxious gases it may contain. Local changes in the weight of the atmospheric column would act on the same principle; and it is conceivable that the electric spark might be an instantaneous source of miasmatic generation, traversing as it does many leagues of space, and acting upon a very complicated mixture.

Dr. Laycock, in his admirable papers on vital periodicity, adverts to the periodic effects of sol-lunar influence upon the earth's electric or magnetic tension, as a probable source of malaria; and our readers are acquainted with the experiments of Dr. Gardiner and the African observers on the existence of sulphuretted hydrogen, and its supposed agency in the production of fever.

The *effect* of miasma upon the system is a more tangible question than its *origin*. M. Guislain,† in a very interesting communication, has pointed out the influence of marsh miasmata in inducing certain acute and continued forms of cerebral disease, closely simulating ordinary inflammation, but only yielding to the free employment of the sulphate of quinine. Dr. Thomas Mayo‡ has directed attention to some forms of masked intermittents, and to another class of cases where periodicity is clearly marked, but where the appropriate treatment is not borne. Some analogous observations have also been made by Mr. Morris,§ of Spalding, and the subject is not destitute of practical interest. We have ourselves met with instances of anomalous symptoms, which the knowledge of ague having formerly existed, or of the patient having lived in a malarious district, has enabled us rapidly to relieve.

An interesting report on the epidemic ague, or fainting fever of Persia, has been drawn up by Dr. C. W. Bell,‖ who considers the disease essentially a quotidian ague. He remarks that the blood

* *Medical Times*, October 14, 1843.

† *Bulletin de la Societé Medicale de Gaud.*

‡ *Medical Gazette*, March 15, 1844.

§ *Provincial Medical Journal*, August 26, and September 23, 1843.

‖ *British and Foreign Medical Review*, October, 1843.

of individuals suffering from other diseases, was invariably of a dark, dusky, reddish-brown colour, and that the serum did not separate from the clot. The urine also of the population generally assumed a very dark appearance. These facts are highly interesting and quite consistent with the evidence of other observers. That the system of every individual is modified during epidemic disease seems now confirmed by much concurrent testimony. Dr. Bell strongly recommends the combination of iron with quinine, stating that the following mixture was successful in more than one thousand cases:—Magnes. sulph., ℥j; Quin. di-sulph., gr. xx.; Ferr. sulph., gr. xij.; Acid. sulph. dil., ♏j.; Aquæ Oiss. ℞. Of this two ounces were given three or four times a day.

On the subject of lunar influence he observes—"I am well aware of the ridicule to which I expose myself in thus attributing such marked effects on disease to the changes of the moon; but the evidence before me is so strong as to have removed all my doubts upon the subject; and I see the effects in such a number of patients who suffer from ague every full and change, that I should feel ashamed did the fear of ridicule prevent me stating a fact of which I am so entirely convinced, and which, if true, is so important."

This candid remark of Dr. Bell will naturally lead us to the very interesting and important question of *Periodicity*, closely associated as it is, though by no means confined to the consideration of intermittent fever. It has engaged, in all ages, the attention of observing and reflecting minds, and the impression that *all* organic acts are subject to regulated periods of quiescence and activity, has never ceased to prevail, though irregularly prominent. The old doctrine of critical days, the phenomena of ague and neuralgia, and other well known physiological facts, receive too frequent illustrations of chronal changes to admit of reasonable doubt; but it is probable that in every disease there are not only diurnal exacerbations and decline, but a term of *natural duration*, the knowledge of which would immensely contribute to the correctness of therapeutic experiment. The question is one of great practical as well as theoretical interest, most extensive in its bearings, complicated in its etiology and relations, and in every respect worthy of increasing attention. No modern writer has laboured with equal ability and success, or availed himself of our actual knowledge of the accessory sciences in the elucidation of the subject, as Dr.

Laycock, of York; and though his interesting papers are not limited to the year of our retrospect, we shall venture briefly to recapitulate the leading points of his very curious and difficult investigations.

The *whole* of the phenomena of individual life are regulated, Dr. Laycock thinks, by one general law; but its operation is most obvious in the phenomena of development. This law determines the recurrence or accession of phenomena by periods of varying duration. The minor periods are a day of twelve hours; a day of twenty-four hours; a week of lunar or twelve-hour days; a week of solar or twenty-four-hour days; and a lunar month. These periods, either simply or multiplied, regulate the developmental phenomena of reproduction. The incubation of all animals, insects, fishes, mammals, is strictly regulated by the law; the exception is when the order of sequence is interfered with by disease or habits of life not natural to the individual, as in birds, confinement—in silk worms, artificial heat, &c. The menstrual period is a developmental or reproductive period; ova are matured in the ovarium at intervals of twenty-eight days, and then separate. Utero-gestation occupies a multiple of the lunar month in all mammals, with certain exceptions. In cows and the human female it is forty weeks; in mares forty-eight weeks; in sows twenty-four weeks, &c. In birds the period of incubation corresponds to those of utero-gestation, as illustrated in Dr. Laycock's tables in his treatise on *Nervous Diseases of Women*. The pointing out of this remarkable connection in the periods of reproduction of all animals, with their analogies in the sex, is, we believe a discovery, to the merits of which Dr. Laycock is justly entitled.

Morbid phenomena would be naturally expected to evidence the same laws. *All* fevers, for example, left to their regular course, follow the periods Dr. Laycock has laid down, as also critical days, as they are termed.* The neuroses, generally, and gout take their periodic type from the same laws. The period of incubation of animal poison is also limited by them, as is the diffusion of epidemics, &c. The hours of the day are not all alike remarkable for the development of vital phenomena, because connected with *meteorological periods*.† Hence a new element of periodicity involves both

* *Lancet*, vol. i., 1842–3, pp. 124–127.

† *Lancet*, vol. i., 1842–3, p. 931.

individuals and societies, and complicates the periods already referred to. This meteorological periodic influence Dr. Laycock denominates *ex*oteric, or exo-periodic; the internal series he terms *es*oteric. The exo-periods are the bi-quotidian, quotidian, lunar week, solar week, lunar month, the four seasons, (as measured by the equinoxes,) and the annual period; and in addition to these Dr. Laycock considers there are also one or two sub-seasonal changes determined by electrical variations, as in February, the beginning of April, the middle of May, end of June, second week in August, second week in December, &c. The major periods are the septennial, and these are made up of the minor periods, which regulate the general development, of which the evolution of the teeth is the best index.* The major periods involving society are astronomical, or rather astrological. They determine, according to Dr. Laycock, the recurrence of epidemics, great electrical changes and their consequences, such as extreme heats, droughts, floods, famines, earthquakes, &c.; also civil and international wars, emigrations, &c.,† he brings under the same laws. The foregoing is a brief outline of this extensive and difficult subject as discussed by Dr. Laycock; but no one can appreciate the evidence on which his conclusions are founded, nor estimate the true merits of the question, without a careful study of the original communications.

A very ingenious lecture was delivered before the Royal Medico-Botanical Society, by Dr. Sigmond, (May 9, 1844,) when he supported the hypothesis that the rarity or density of the atmosphere, as produced by its four diurnal tides, materially influenced diseases of the respiration and of the nervous system. He regarded them as the cause of the periodicity observed in asthma, fever, epilepsy, and many other diseases, and suggested the importance of medical men registering the state of the barometer at least twice in the twenty-four hours. In the United States every military and naval surgeon has been called upon to pursue this plan; and the consequence would be, that the attempt of Professor Forbes, discussed before the British Association, to give us a regular formula of the atmospheric tides, would be carried out; and if with this each practitioner gave his observations on the periodicity of disease, tables of very valuable and practical information might be formed.

* *Lancet*, vol. i., 1843–4, p. 255.

† *Lancet*, vol. 1, 1843–4 p. 430.

Mr. George Ross* thinks that from our present knowledge of animal chemistry, we could easily determine the periods of renovation to which the system is obedient, and hence deduce the laws of periodicity in fevers, assuming that these laws are depending on *es*oteric changes. Hence the more rapid the waste, the shorter the period of change. Thus a quartan ague may change to a tertian or to a quotidian fever, but the danger is increased because the waste is greater, while, if a quotidian assume the tertian or quartan form, the danger diminishes, the fever abates, the urinary deposits are more scanty, and a speedy recovery is indicated. Mr. Ross doubts not, and his opinion is entitled to respect, that we shall soon be able to determine with precision the periodic accessions of fevers and other analogous diseases, by simply attending to the amount of the azotised secretions. Another theory is suggested by Dr. P. H. Williams, in his Prize Essay on the Nature and Objects of Medical Science, viz., that the gradual decomposition of the morbific agent may account for intermissions in ague, the action of the poison being arrested during the reaction, and again proving active when this has subsided. The period would depend on the rapidity of decomposition, and the number of fits on the quantity of the poison. Bark, arsenic, &c., he regards as useful, by preventing anormal chemical decomposition.

The ever fertile Raspail† imagines that the morbific agent renders the blood unfit to be absorbed and aspired by the tissues, which cease to elaborate, produce less caloric than they yield, and hence the phenomena of the first stage. A return of normally-constituted blood is the cause of the reaction. The same observer, still more recently, after concluding that nine-tenths of human diseases are referable to parasitic animals, asks whether intermittence may not depend on these reptiles applying themselves to their work of destruction, or, as tired of their labours, they shall lie by to digest the fluids of which they have drained the tissues. The answer we must leave to the followers of this most gifted but erratic investigator.

A singular series of observations, illustrative of the antagonism of typhoid and intermitting fever, has been made by Dr. Boudin,‡

* *Lancet*, February 17, 1844.

† *Medical Times*, October 14, 1843, and January 19, 1844.

‡ *Gazette Medicale de Paris*, Novembre 25, 1843.

of Marseilles. Two infantry regiments have presented the most opposite conditions, in the same locality; the one being liable to typhus and not to ague, the other the reverse. The first illustrates the effects of local influences, the second that of the district from which it had been removed. Dr. Boudin has constantly observed that when regiments arrived at Marseilles from the marshy localities of Corsica, the Morea, and Algiers, they remained untouched by typhoid fever, although it decimated the resident garrison; and, on the other hand, regiments which proceed to Algiers from Marseilles yield many cases of typhoid fever for months after their arrival, although that disease would be sought in vain amongst any other of the civil or military residents of the place.

It is well known that typhus becomes endemic in districts formerly aguish, and instead of antagonists to each other, we should rather regard them as depending upon some modification of the same morbific agent. The subject is, however, interesting and obscure, and the great length of time which a malarious principle exerts its influence upon the system, the strange irregularities of its manifestation, and the long periods of latency it not unfrequently presents, are all facts very imperfectly, if at all understood, and evince the poverty of our present knowledge of the subject.

Puerperal Fever.—A very severe epidemic of puerperal fever has prevailed at the Paris Maternité during the past year.* Fifty-four cases were admitted into the Hôpital Necker, under Dr. Bouchut, who has made them the basis of a very interesting statement. Dr. Lee, as is well known, considers the disease as essentially dependent on inflammation of the womb and its appendages, and its different varieties to be referable to the serous, muscular, or venous tissues being principally affected. This view of its pathology, though very ably maintained, is not however generally adopted; and among others, Dr. Bouchut regards it as the expression of a general, and not a local cause, acting upon a peculiar individual condition at a time when the weakest impressions determine multiplied disturbance, affecting several organs at once, and tending rapidly to suppuration and death. When, in fact, we consider the interesting modifications of the

* *Gazette Medicale de Paris*, Fevrier 10, 1844.

general health during the pregnant state; the well-ascertained progressive increase of fibrine in the blood; the singular changes taking place in the osseous system of the female; the peculiar condition of the whole organism after the wonderful process of parturition—and combine these considerations with the epidemic prevalence of puerperal fever; the coincident existence of low typhoid fevers; the frequency of erysipelas and other analogous conditions; the power of communicating the disease; the variety and extent of the morbid appearances; the great and rapid tendency to suppurative action; and the remarkable changes in the circulating fluid detected after death—we feel it difficult to regard the affection in any other light than as ordinary fever, modified by the circumstances to which we are alluding. Dr. Bouchut states that on examining the blood in a rapidly fatal case, where no local cause of death could be detected, there were at least ten times as many white corpuscles as in healthy blood, and that in eight other instances he has recognised the same fact.

Some very practical and important communications on the contagion and treatment of this interesting disease have appeared in the *Provincial Medical Journal*, from Mr. Fisher, of Bungay,* Mr. England, Mr. Storrs, of Doncaster,† Mr. Elkington,‡ and Mr. Crowfoot,§ all tending to demonstrate the fact of the affection being too frequently communicated by the medical attendant, and strongly insisting upon the precautions every midwifery practitioner should take, whenever the slightest suspicion may exist of his being the possible vehicle of contagion. A very large amount of distressingly confirmative evidence can now be referred to, and no theory as to the origin or nature of the disease can justify inattention to facts so convergent and multiplied. It should be remembered that erysipelas, and probably all febrile conditions to which the term malignant is applicable, are to be regarded as capable of inoculating puerperal fever. M. Trousseau‖ states that the treatment recommended by practitioners of the preceding century, of administering emetics two or three times during the first twenty-four hours, and drastic purgatives for some days subse-

* *Provincial Medical Journal*, November 4 and 11, 1843.

† *Provincial Medical Journal*, December 2, 1843.

‡ *Provincial Medical Journal*, January 13, 1844.

§ *Provincial Medical and Surgical Journal*, April 3, 1844.

‖ *Gazette Medicale de Paris*, Mars 9, 1844.

quently, has proved very successful in his hands. Mr. George May, of Reading, lays great stress upon the frequent use of injections of an emollient or antiseptic character, in all cases where the discharges are offensive, as well as during the progress of disease.

Puerperal Convulsions.—We may here, for the sake of convenience, notice the interesting discovery of Dr. Lever,* that the urine of women attacked with *puerperal convulsions*, or presenting their precursory symptoms, is constantly *albuminous*. The fact is one of great practical value; and Dr. Lever has ascertained that this condition is not an attendant upon uncomplicated pregnancy. Other observers have since confirmed the correctness of Dr. Lever's observation, who adopts Mr. Robinson's theory of its dependence on pressure on the emulgent veins.

Two very practical papers, by Drs. Halpin and Johns, occur in the *Dublin Medical Journal* for September, 1843. These observers confirm the now generally received conclusions, that puerperal convulsions are almost limited to *first* labours and to *natural* presentations, and that the most effective treatment consists in free general depletion with purging, with nauseating doses of emetic tartar, and the induction of artificial labour, if it can be accomplished without violence; they also coincide with Dr. Collins on the danger of turning under these circumstances. Dr. Johns lays particular stress upon the swelling of the face and superior extremities, as the most uniform premonitory symptoms—a remark made by many others, and quite in accordance with the albuminous state of the urine.

The value of counter-irritation over the sacrum, and of sedative injections, has been adverted to by Mr. Paterson;† and my esteemed and experienced colleague, Dr. Smith, has much confidence in opium so administered.

Erysipelas.—Dr. J. A. Wilson‡ maintains that idiopathic erysipelas of the head and face is a disorder essentially constitutional, specially determined to certain structures, fulfilling within a given period of time, from eight to twelve days, its operation on the skin and

* *Guy's Hospital Reports*, October, 1843.

† *Medical Gazette*, June 7, 1844.

‡ *Lancet*, March 23, 1844.

on the general system, and should therefore be classed with the eruptive fevers. The wonderful effects ascribed to iodine, nitrate of silver, &c., are, no doubt, in many instances, depending on their use coinciding with the natural termination of the disease, a correct knowledge of which would be a continual rebuke to the vanity which links the last prescription and the cure invariably together. Dr. Wilsons's views would indicate the impropriety of repellent applications, and the wisdom of expectant treatment. Mr. Chippendale and others are dissentient to these opinions, and place confidence in the power of remedies in shortening the duration of the morbid phenomena.

Vaccination.—The important subject of vaccination has been annually discussed by the profession from the period of its first introduction, and it is still a question involving many points of theoretical and practical interest. From a very elaborate memoir, in answer to a series of questions addressed by the Academy of Science, of Vienna, to the Rector of the Faculty of Medicine,* we glean the following remarks:—

After fully admitting the protective and mitigating power of vaccination, the importance of acute reactive fever being established on the seventh and eighth days is strongly urged, it being regarded as the only test of future security. Jenner entertained a similar idea. It has been observed that vaccination from old to younger subjects excites greater reaction than under ordinary circumstances; female children, and those of light complexion, are said to transmit a milder form of lymph than males and those of opposite temperament. The constitutional benefit derived from vaccinating a feeble from a vigorous child is asserted to be great, both in accclerating growth and apparently interrupting hereditary developments. The popular impression, that different diseases are inoculated by vaccination, is considered as well-founded, and the selection of lymph from a young and healthy source should at all times be carefully attended to.

On the much debated question of revaccination, the Reporter advises its repetition in all cases at the age of seven; but that after the fortieth year it is unnecessary, reinfection from variola being almost limited to between the eighth and thirtieth years.

* *Journal de Medecine de Beau.*, Juin, 1843.

M. Bousquet* thinks re-infection seldom occurs before the twelfth year, and is favourable to revaccination, while M. Gaultier de Claubry,† from very extensive data, denies its necessity, and affirms the undiminished activity of the transmitted vaccine. M. Villaret, from observations on the French Army in 1839-40-41 and 42, advises revaccination every ten years.‡ M. Lossetti, of Milan,§ has also instituted extensive statistical inquiries on the same subject, and says that we cannot deduce the probability of future infection from the character or number of the cicatrices. He also concludes in favour of revaccination; and from the analysis of 1411 cases affected with variola after vaccination, proves that the liability to reinfection is less limited by age than would be inferred from the Austrian Report.

It is too evident from the above brief statements that this important question is yet surrounded by difficulties; but it is equally a matter of deep congratulation that the inestimable benefits of Jenner's discovery are not dependent upon the issue of the discussion.

Scarlatina.—The value of carbonate of ammonia in scarlet fever has been attested by several observers, and during the past year by Dr. Picken, of Dublin,‖ and Dr. Ricken, of Belgium; while at St. Petersburgh it is said to have been employed by the German physicians without any evident advantage. Mr. H. Freke suggests that it may act by taking the place of urea, and thus supply the natural stimulus to the renal functions; and Messrs. Herdenreich and Heim have observed, that in cases of scarlatina an ammoniacal alkali is deposited on the skin, and hence offer another explanation of its efficacy. Whatever the theory, the amount of practical evidence preponderates in favour of the administration of ammonia in scarlet fever.

Some additional data confirmative of the prophylactic powers of belladonna have been adduced by Dr. Van Berchem, of Brussels;¶ and, however difficult to reason upon the fact, moral if not physical

* *Séance*, Septembre 26.
† *Medical Times*, 1843.
‡ *Medical Times*, January 6, 1844.
§ *Gazette Medicale de Paris.*
‖ *Medical Times*, July 29, 1843.
¶ *Archives de la Medecine Belge*, Septembre, 1843.

advantages may result from its exhibition. Dr. Van Berchem would extend its application to measles and other epidemic conditions. We may also refer to the very admirable essay of Dr. Kennedy, on the epidemics of scarlatina, which prevailed in Dublin from 1834 to 1842. It is a volume full of sound practical information.

M. Trousseau* directs attention to the old practice of urtication in cases of recession of eruptive disease.

Measles.—Five fatal cases of croup following measles have been related by Dr. Charles West.† This insidious complication may be suspected, he says, wherever there are universal drowsiness, disinclination to swallow or difficulty in the act of deglutition, reluctance to speak, or alteration of the tone of the voice, even though there be no croupy cough or stridulous breathing. The suspicion would be almost raised to certainty if the gums have a spongy or ulcerated appearance, or if the tongue be preternaturally red or apthous.

Glanders.—That glanders may be communicated by inoculating with the blood of a glandered animal as well as with the morbid secretion, has been satisfactorily demonstrated by the experiments of M. Renault.‡ Three well-marked and fatal cases of this formidable affection have been recorded by Mr. Hamerton.§ From one an ass was inoculated and died on the fifth day. The disease seems only capable of transmission by direct contact, and when so communicated from man to man loses nothing of its virulency. Dr. Hughes‖ suggests the free administration of mercury, and giving an early exit to all accumulations of matter. Some change of treatment should certainly be attempted. This and the succeeding disease are instances where experiment is not only justifiable but imperative.

Hydrophobia.—Would we had anything to record on the subject of hydrophobia, which would justify the slightest hope of successfully contending with this most awful of human visitations. The past

* *Journal de Medicine*, September, 1843.
† *Medical Gazette*, September 1, 1843.
‡ *Provincial Medical Journal*, July 8, 1843.
§ *Dublin Journal of Medical Science*, July, 1843.
‖ *Guy's Hospital Reports.*

year has not been exempted from furnishing several painful examples; nor has it been silent in suggesting some practical indications, and to the latter we shall for a brief moment direct the reader's attention. The operation of tracheotomy was, we believe, first suggested by Mr. Mayo, and has more recently been recommended by Mr. Turner,* with the view of relieving the fearful struggles depending on spasm of the larynx, and, by the prolongation of life, to give the system a chance of wearing out the disease.

Dr. Hutchinson† urges the trial of large doses of belladonna *after* the operation of tracheotomy, and his suggestion is worthy of attention. The trachea should, we think, be opened in all cases, whatever other means may be employed; it could scarcely fail to mitigate some of the more agonising conditions. Mr. Yearsley has proposed excision of the uvula upon a similar principle.

In Dr. Foley's case‡ considerable relief was afforded by the tobacco injection. He also refers to a remedy brought to England from Bohemia by the Earl of Clanwilliam, which is there regarded as a certain preventive of the disease. It was tried some years ago in London on three soldiers of the Royal Horse Guards, all of whom escaped, while two horses, bitten by the same dog, and to whom it was not administered, died.

A very sensible letter, we believe from Dr. Clanny, of Sunderland, appeared in the *Northern Times* of September 12, 1843, strongly advising salivation in all suspected cases. The parts are to be well washed and dressed with savine ointment; the system kept cool by saline aperients; from ten to fifteen grains of blue pill daily administered; and blisters applied to the fauces, dressed with equal parts of blue ointment and simple cerate, till copious salivation ensued. The author refers to several instances where these means were apparently successful.

The latest remedy we have heard of is the active principle of the viper's poison, *echidnine*, discovered and recommended by Prince Louis Buonaparte, which is to be injected into the veins of hydrophobic patients. We suppose the idea is founded upon what was suggested some years ago, when experiments were making on the

* *Lancet*, December 9, 1843: British Veterinarian.

† *Lancet*, May 24, 1844.

‡ *Medical Times*, January 1, 1844.

woorari poison, viz., to suspend the voluntary life and give time for the morbid action to subside; and we anticipate it will meet with an equally speedy oblivion.

Blood.—We do not consider it to be our duty to enter upon the many interesting, but as yet unsatisfactory investigations, relating to the chemical and microscopic characters of the blood, as the subject will be ably discussed by Dr. Budd, and because we feel quite conscious of personal incompetency to form any accurate opinion of the real value of observations requiring much special knowledge and experience for their just appreciation. Many interesting views and suggestions will be found in Mr. Addison's* recent communication. He has imparted to his statements a clearness and precision not always the characteristic of microscopic observers, and evinces a soberness of thinking which entitle his conclusions to our attention and respect. "All the future advancing steps of physiological and pathological anatomy will be accomplished," says Mr. Addison, "by those who will carry with them the microscope and the test-tube to the dissecting room, and seize upon the objects which adhere to and leave their stain upon the bright blade of the scalpel." It is at all times refreshing to hold converse with minds ardent in the pursuit of truth, and yet imbued with a just estimate of the narrow limits of all human inquiries. In no branch of physical investigation are these qualities more essential, than in the attempt to fructify and interpret the ever-opening wonders of microscopic discovery.

Mr. Wharton Jones† has pointed out a very ingenious method of determining whether the blood is buffy, or not, from the examination of a very minute portion of this fluid. It consists of quickly enclosing a drop between two pieces of glass, and observing with the naked eye the quickness with which it assumes a mottled appearance, and the smallness or largeness of the interspaces. In buffy blood the mottling is almost instantaneous and the interspaces large, while in healthy blood it is delayed for half a minute or more, and the reticulation is minute.

A very interesting and successful case of transfusion is recorded by Dr. Prichard in the *Provincial Medical Journal*, July 29, 1843.

* *Transactions of the Provincial Medical and Surgical Association*, vol. xii.

† *Edinburgh Medical and Surgical Journal.*

It was practised in the case of a gentleman reduced by protracted and severe dyspeptic symptoms to extreme inanition and threatened with fatal syncope. All other means failing, sixteen ounces of healthy blood were injected, with immediate benefit, and the gradual restoration of appetite and strength. Dr. Prichard believes that many lives might be saved by a similar plan, and perhaps, he says, transfusion may become hereafter as important a remedial measure as the detraction of blood has been since the time of Hippocrates.

The almost trite, but ever-important subject of bloodletting, in accordance with the principles so clearly demonstrated by Dr. Marshall Hall, has been ably discussed by Mr. W. Barlow.* The general practical adoption of admitted principles is singularly slow. Fresh knowledge is very tardy in amalgamating with our daily habits, and long nestles in the mind before it habitually influences our practice; hence the necessity for the frequent repetition of the same precepts.

Purpura.—The pathology of purpura hæmorrhagica has received no elucidation during the past year. Dr. Starr, of Kettering, relates an interesting case† rebellious to acids and astringent means, but rapidly benefitting under the saline treatment, as recommended by Dr. Stevens in continued fever. Dr. Starr thinks that medicines in an effervescing state give rise to considerable electrical development, and thus exert a salutary influence upon the whole nervous system. The great preference or singular repugnance to medicine in this form displayed by particular individuals may be dependent upon this principle.

Malignant Disease.—The subject of malignant formations has excited a good deal of attention and laborious research during the past year, but we confess ourselves amongst the number of those who are not conscious of that increase of our actual knowledge upon the subject, which the novelty and extent of modern observation might lead one to infer.

The "cell theory," we are told, is about to unravel the mysteries of disease as well as health; and it requires no prophetic vision to

* *London and Edinburgh Monthly Journal*, December, 1843.

† *Provincial Medical Journal*, October 7, 1843.

perceive, says a recent writer, that the remarkable advances which microscopic discoveries have effected in physiology, are destined to produce corresponding important changes in the character of pathological science, and to widen the limits of those narrow bounds within which the routine morbid anatomist has hitherto been accustomed to circumscribe his enquiries. It cannot be doubted, he proceeds, that the most fruitful materials for the successful extension of pathological science lie hid in the integral elements of structure, and of these the elementary type seems to be a slender tripartite cell. Now, without daring to express the slightest doubt as to the solidity of the foundation upon which these bright anticipations are to be realised, we proceed to state, that Dr. Thomas Williams,* in an essay on the pathology of cells, concludes:—"That as science advances, the probability becomes correspondingly stronger; that general laws will be developed on which to account for the production and unmanageable progress of malignant disease; that the facts and principles already unfolded warrant the inference that *malignancy* is originally synonymous with a local perversion of nutrition, and that after a certain continuance of the process, a reaction of its influence upon the general system occurs, after which all hopes of recovery from surgical removal of the part are blighted; that the cells of malignant growths are only monstrosities of the normal cells, produced by a perversion of the moulding agency during the process of growth; that the degree of malignancy is proportionate to the simplicity and uniformity of the ultimate elements of the tumour; hence the reason why encephaloid disease is less under the control of the system than scirrhus."†

Dr. Roquetta very vaguely assimilates the cancerous diathesis to that of chlorosis and scrofula, especially the former, and strongly urges the importance and efficacy of internal treatment.‡ Dr. Tanchou, in his recently published work, also adduces very extensive evidence to prove that a cure may be obtained without having recourse to the knife or the cautery. Both observers unite in recommending, in the early stages, small and repeated bleedings,

* *Guy's Hospital Reports*, October, 1841.

† See an able digest of the cell theory in the *British and Foreign Medical Review*, January, 1844.

‡ *Archives de Therapeutique*, Mars. 1844.

with the internal use of iodine, steel, quinine, alkalies, mineral waters, warm baths, acidulated drinks, and mild regimen; with the steady use of iodine, mercury, arsenic, chloride of zinc, and other metallic preparations, in the later stages. Now there is nothing new in all this, and the only question of interest is, whether these writers are correct in their reported cures, and if so, how they have attained the power of successfully employing remedies which in the hands of others have almost universally failed.

The influence of metallic preparations upon cellular life, and the inferior vitality of cancerous growth to that of the healthy textures, certainly favour the theoretical assumption of possible good, and perhaps greater perseverance in their use, and administering them in a highly diluted form might lead to more generally successful results. At all events failure should not depend upon deficient assiduity; and with statements of success so strong, and apparently well founded, as those we are now canvassing, the practitioner is called upon to reserve his faith, and leave nothing untried. It would seem from M. Tanchou's researches, that the number of cancerous affections is increasing in England, Germany, and France, and that civilisation is the great exciting cause.

Mr. Tuson,* of the Middlesex Hospital, has continued his meritorious investigations upon the curative powers of the various combinations of chlorine, particularly the ter-chloride of carbon, and he cannot doubt that, both as external and internal remedies, they exert a remarkable modifying power over cancerous diseases. In forty cases he instituted enquiries as to the habitual use of salt, and found that salt food was disliked, and that it was very sparingly used as a condiment. The fact is curious, and worthy of further investigation, particularly as great importance is ascribed to this agent in the nutritive and disintegrating processes. Mr. Wilson, of Manchester, thinks he has seen the progress of cancer arrested, and even suspended for years, by the use of taraxacum; and Dr. Rowe, of Chigwell, states that great benefit has resulted from the chloride of lime lotion, with warm bread and water poultices.

The subject of malignant cutaneous diseases has been ably investigated by Dr. Byron,† who thus defines the diagnostic

* *Lancet*, January 13, 1844.

† *Dublin Journal of Medical Science*, September, 1843.

differences between fungoid and scirrhous affections of these textures. Fungoid disease usually commences in the sub-cutaneous cellular tissue; is lobular, vascular, elastic, and of a dusky dull colour; it is less circumscribed; rapidly increases in volume, especially after ulceration; is slow to contract adhesion with the skin, does not extend itself in the line of the absorbents, and is most commonly observed in young subjects; relapses are also frequent after operation. Scirrhus, on the other hand, commences in the skin; is firm, fibrous, slightly vascular, defined in outline, and rarely of large dimensions or the seat of hæmorrhage; adheres quickly, and progresses slowly after ulceration. It seldom occurs before the twenty-eighth or thirtieth years of age, usually extends itself in the line of the absorbents, is slow in vitiating the system, and is often permanently cured by excision or otherwise. In fungoid formation the microscopic elements are globules, not always distinctly cellular, and caudate corpuscles. In scirrhus they consist of juxta-posed nuclear cells, but no caudate corpuscles.

Rheumatism.—Professor Schönlein's* clinical lectures present a very able and interesting illustration of German pathology and practice. Dr. Schönlein is evidently no ordinary man, and it were much to be desired that we could boast clinical instructors of equal capacity and zeal. I shall here avail myself of a few of his remarks upon rheumatism. The continuance of an acid state of the urine and perspiration he regards as a certain indication that the disease is not cured. Other secretions are also characterised by a predominance of acid; and he mentions an instance where an ulcerated surface was covered during the night with an earthy crystalline crust, consisting of an excess of uric acid with ammonia and soda. There can be no reasonable doubt of uric acid existing in the blood, though we are not aware of any experimental proofs. Such a fact would explain the admitted use of alkalies in this disease, and of other remedies which influence the amount of the excretive functions. Dr. Furnival has very lately stated his experience of the great advantage of an alkaline treatment; and perhaps in non-syphilitic cases the value of the hydriodate of potass may be dependent upon the alkaline base. Dr. Schönlein particularly adverts to the complication of miliaria in rheumatism, as one of

* *Lancet* and *Medical Times.*

considerable interest and importance. He speaks favourably of the effect of alkaline baths, frequently repeated; or, if these are contra-indicated, of alkaline lotions containing half a drachm to half an ounce of caustic potass to the pint. He denies the modern discovery of cardiac complication in rheumatism, but states it to be an old theory which long ago found its way from Italy into the north. The tendency to valvular disease he considers to be less in cases where the urine is charged with lithic deposits, remarking that these are uniformly less copious in women than in men. In the former they are often replaced by a thick acid mucus, probably cystine, and combined with this there is frequently an irritating acid vaginal discharge.

Some interesting observations are made on rheumatism of the abdominal muscles, chiefly of the recti and pyramidales, which often gradually extends to the peritoneum, and evinces a remarkable tendency to the formation of matter. The elective site of the abscess is over the bladder, and the contents are apt to have a strong urinary odour. Professor Schönlein asks whether suppuration is limited to those individuals who have pyramidal muscles? Such a fact, he says, would corroborate the pathological law, that every organ whose existence belongs to a lower degree of development, is particularly disposed to disease; that the morbid process corrects, as it were, what has been omitted during the evolution of the body, and forms a kind of supplementary evolution. We merely adduce this speculation as a specimen of German transcendentalism.

M. Monneret* has carefully tested the asserted value of quinine in thirty-two cases, and concludes that while it exerts an unquestionable influence upon the local symptoms, especially the pain, its action is seldom durable, and that it possesses no superiority over other remedies. He thinks it rather predisposes to cardiac complication; that it does not lower the pulse, while it sometimes induces a special form of poisoning, commencing with anomalous nervous disturbance, followed by gastro-intestinal irritation, and, lastly, by general typhoid depression. The effect of excessive doses of this remedy on the blood has been clearly ascertained in animals, and some sad instances of its irrational employment in man have been recorded during the past year.

* *Journal de Medecine de Beau*, Janv. et Fevr., 1844.

The nitrate of potass in large doses has been tried by Drs. Monneret and Bennet,* with conflicting results. The former found it valueless; the latter considers it a safe and efficient remedy. Dr. Bennet states that relapses are rare, and that cardiac complications seldom occur during its use, while no injury results to the general health. He has found it extremely valuable, in the dose of an ounce daily for a week, in the slight forms of rheumatism common in damp weather, and manifested by local more than general symptoms.

Dr. Pereyra, of Bordeaux, after various comparative trials, gives the preference to the following formula:—Pulv. resin. guaiaci, ʒj.; Fol. aur. pulv., ʒss.; Morph. acet. gr., ¾ Misce. Divide into sixteen powders—one to be taken every two hours.

Rickets.—It would appear from the researches of Marchand,† that rickety children pass five or six times more earthy phosphates in the urine than what is normal. Now, Berzelius has shown how easily the phosphate of lime is dissolved by lactic acid, which seems generated in excess in strumous diatheses. While the alkaline salts are abundant, this excess of lactic acid is neutralised, but if otherwise, it dissolves the calcareous phosphates. The practical indication in rickets is, therefore, the steady administration of alkalies, and avoiding those substances, as sugar, starch, and gum, which would favour the formation of lactic acid. Chemistry thus confirms what experience has long demonstrated, that alkalies with animal diet form the basis of all anti-scrofulous treatment.

A particular affection of the skull in children has been described by Dr. Elsaesser, in his contributions to the physiology and pathology of early childhood, under the term of the "soft occiput." It presents all the gradations from softening and thinning to perforation of the bone, and seems dependent on the effects of pressure in weakly constitutions. The secondary consequences are cerebral irritation and convulsive seizures. Dr. Elsaesser states that he has met with forty cases in five years, which must indicate some endemic peculiarity, the condition described being apparently but little familiar to British practitioners.

We may here as well allude to a very opposite condition of the

* *Lancet*, January 15, 1844.

† *Medico-Chirurgical Review*, January, 1843.

cranium, and one of a very singular character, closely associated with the pregnant and puerperal state. Professor Rokitansky, of Vienna, first described it about two years ago, but more recently it has been minutely investigated by M. Ducrest, of Paris.* All in whom it has yet been discovered died in child-bed; and out of 231 crania, in 90 this kind of osteophyte was found in different stages of progress. It consists of a remarkable thickening of the skull, almost limited to the inner table, without any morbid condition of the dura mater or corresponding portions of the cranium. Young women appear principally affected, and its presence gives rise to no particular symptom. The fact is one of considerable interest, but at present most obscure in its etiology and pathology. Perhaps some morbid change in the cranial bones of the character now described, may be the exciting cause of puerperal convulsions and other forms of nervous disturbance; but the subject requires more extensive investigation.

Scrofula.—Professor Negrier† has continued his researches on the use of the extract and decoction of the leaves of the walnut tree, internally and externally, in cases of scrofula; concluding most favourably as to its curative powers. The effect, he says, is slow, but satisfactory, and the after relapses few. As a local application in strumous ophthalmia and open sores, he regards it as superior to all others. The remedy is certainly deserving of additional experiments.

Diseases of Old Age.—The general and special pathology of old age has not yet received all the study and attention which it deserves. An important memoir by M. Prus,‡ and one not less excellent by MM. Hourman and Dechambre,§ contain much interesting detail upon the subject; and in the writings of Halford and Carlisle many practical observations are recorded. But there is yet room for additional research, and we shall briefly allude to the more recent labours of M. Beau,‖ on the aged females of the

* *Memoires de la Societé Medicale d'Observation*, tome ii.; also *London and Edinburgh Monthly Journal*, March, 1844.

† *Archives Generales de Medecine*, Fevrier, 1844.

‡ *Memoires de l'Academie Royale de Medecine*, vol. ix.

§ *Archives Generales de Medicine*, Aout., 1835.

‖ *Journal de Medecine de Beau*, October, 1844.

Salpetrière, as another valuable contribution to this department of inquiry. In advanced life disease seldom presents itself in an uncomplicated form; the organic sympathies are less easily excited, and but little general disturbance may attend fatal organic changes. There is also a gaiety of feeling and expression singularly at variance with the visceral conditions. Both the diagnosis and prognosis are therefore more difficult and uncertain. Delirium, Dr. Beau observes, is often evidenced by irregular *acts*, and terminates by irregular *expression*—the reverse of what is usual in adults. Inflammation of the parotid glands is a frequent complication of all low febrile affections. Old people seldom perspire, and sweating is unfrequent during either fever or phthisis. Acute disease is seldom announced by distinct rigor. Dr. Beau has never noticed subsultus, or the irregular muscular movements so common during fever in adults. Approaching prostration is often ushered in by simple dryness of the tongue; and a powdery state of the nostrils is an indication of declining strength. Seasonal changes exert increased influence, and cold is the great enemy of advanced life.

In his section on the special pathology of the aged, Dr. Beau observes, that chronic peritonitis has never been, in his experience, tubercular, but constantly connected with cancerous sub-peritoneal deposit. There appears to be a singular incompatibility between cancer of the stomach and senile bronchitis. However dyspeptic and cachectic the patient might appear, the fact of bronchitis would almost involve the absence of any malignant gastric affection. We are not aware of the experience of any other observer upon this very unaccountable fact. The lower bronchial tubes are principally affected in the aged. Pleuro-pneumonia is a more frequent complication than in the adult, and when severe, presents all the general symptoms of phthisis. The progress of the latter is usually very slow, with little febrile disturbance; the deposit small in extent, and for the most part limited to a single lung. Perspirations, or laryngeal and gastro-intestinal symptoms, are seldom present. Dr. Beau has particularly investigated the nature of the cicatrices and puckerings so frequently seen at the summit of the lungs, and has no doubt of their tubercular origin. Out of 196 *post-mortem* examinations of non-phthisical individuals, they were absent only in three.

Pneumonia also presents some very rapid and remarkable changes in its physical signs and general symptoms, without a knowledge of which the observer might well doubt his powers of correct observation. This saccaded course of the disease may be repeated two or three times in succession, and can only be explained by supposing that in the aged, inflammatory extra-vascular effusion does not always take place, and that the actual condition is one of great vascular obstruction and fulness, which may of course admit of easy alternations. Dr. Beau suggests whether in the latent form of pneumonia, the inflammatory products are not extra-vesicular, and in the acute form inter-vesicular; but either supposition involves equal difficulties. Pneumonia is, according to this observer, always latent if it occur in the second lung during an acute affection of the first, as also in the case of a relapse in a lung previously affected. Pleurisy is rare, but much more fatal in the aged. Acute pericarditis Dr. Beau has never met with; and when the rational signs are present, they are usually associated with polypous concretions in the heart. Dropsy, with albuminous urine, is also comparatively rare. The general character of the facts detailed by Dr. Beau admits of applications in the estimate of disease in old people, far beyond the illustrations of the peculiarities now given; and an accurate acquaintance with the same disease, as manifested in the three great divisions of human life, would materially contribute to correct ideas of its essential nature and etiology. This remark peculiarly applies to the history of tubercular formations.

Climacteric Disease.—Dr. Kennedy,* in a very practical communication, points out that the group of symptoms first accurately described by the late Sir Henry Halford, under the term of "climacteric disease," is by no means limited to the advanced period of life, but is still more frequently met with in persons between twenty and thirty years of age. Indeed, Dr. Kennedy thinks that few individuals wholly escape. We can recognise in Dr. Kennedy's description a class of cases which has often perplexed and disappointed us; and to be able to view them intelligently, to have a rational estimate of their probable duration and progress, to know them in fact, if only negatively, is a gain of no small satisfaction to the practitioner, and advantage to the patient. Every

* *Dublin Journal of Medical Science*, May, 1844.

careful observer must have encountered many forms of disease not pictured in his nosological chart; and the slightest glimmering of light to track more clearly their obscure and unaccustomed features, is a boon of no ordinary value. Dr. Kennedy's observations are worthy of attentive consideration.

The progress of medical science, Dr. Thomas Mayo very justly observes,* must be from individual instances to general propositions. In practice the direction of thought is the reverse. Another important process is, to individualise cases of the same kind: we deal with facts to discover their differences. The first is statistical, and would lead to indiscriminate practice; the latter unbinds the facts and fits them for exact use. Hence the value of carefully recorded cases. We want the practice of the individual physician. How many eminent men die without a memorial beyond the decaying recollection of their contemporaries. How gladly would we learn how a David Pitcairn, a Warren, or a Baillie, treated certain combinations of symptoms. Each mind makes its own application of general laws, and either correct reasoning or happy experiment conducts to fortunate therapeutic results. Every man has found out something in the practice of his art worth imparting to others; and to get at the practical experience of any man in its bedside detail and application, is to derive many useful hints, and to make many useful additions to our own knowledge. We abound in monographs and general treatises, but we want the autograph of the busy practitioner—those results which experience has thoroughly sifted and adjusted for immediate application. The produce of a life's labour should not be entombed, but recorded, however briefly, for those who succeed. How strongly do we feel the truth of these reflections when looking round upon an assembly like the present. How much unuttered wisdom does it not contain. What invaluable hints might be given to their youthful successors, by those now grey in years and ripened in experience. This is the literature we want—the brief but full-fledged conclusions of a long-practised mind, of a matured unselfish heart. Let those so richly freighted recollect the value of the treasure entrusted to their keeping, and feel it to be both their privilege and duty to bequeath it to others.

* *Medical Gazette*, January 26, 1844.

A talented and eloquent essay, from the pen of Dr. Marx, on the decrease of disease through the progress of civilization, has been recently published in the *Medical Gazette*. The author is evidently an enthusiast, and fond of the brighter side of his glowing picture; and in Dr. Willis, the translator, he has met with an able and kindred coadjutor. Dr. Marx concludes, that in proportion as morality and science advance, are the means multiplied by which human life is assured against enemies from within and from without; he does not, we think, sufficiently estimate the attendant evils of a civilizing process.

Hygiène.—It is by increased acquaintance with the all-important subjects of ventilation, draining, cleanliness, proper modes of building, diet, and all other influences affecting our daily life and habits, or rather by the amount of effort which is made to rectify and remove every known cause of injury to the moral and physical well-being of the community, that we may reasonably expect materially to influence the standard of general health. Great Britain, with all her social advantages, has been sadly negligent of hygienic regulations; but, within the last few years, surprising exertions have been made, and are still making, to wipe away our reproach, and to lessen, and, if possible, to remove the evils from which we have so long suffered. We do not intend to enter upon so important and extensive a subject, but shall merely observe, that in the writings of Farr, Chadwick, Reid, Holland, Noble, Duncan, Alison, Perry, and Wilde, the past year has been remarkably fertile in all that relates to the sanitary welfare of our population. The greatest and most successful applications of knowledge will be those which have for their object the prevention rather than the cure of disease. We may reasonably boast of the triumphs of medical skill, but we must hail with far deeper satisfaction all measures calculated to diminish the necessity for its exertion. To those who devote their energies in unveiling the great sources of disease, and in multiplying the conditions of health, the public are peculiarly indebted; and the amount of unrewarded toil which genuine philanthropy has prompted, will ever constitute a glorious characteristic of the age in which we live. Here Science and Christianity are striving together, and the fruit will be the blessing of him who was ready to perish.

DISEASES OF THE HEAD AND THROAT.

Muscæ Volitantes.—The true nature and seat of muscæ volitantes are, we think, at last decided by Dr. James Stark, of Edinburgh,* who has performed a most ingenious and demonstrative set of experiments upon himself. He clearly proves that they are produced by the movements of the mucous globules on the conjunctival surface of the cornea, acted upon by the elevation or depression of the lids, and modified by the amount of moisture, evaporation, &c. A microscopic examination of healthy eyes discovered the same bodies in every instance, and the whole phenomena were artificially produced as in the living subject. Their visibility, therefore, depends upon morbid irritability of the retina or brain. A correct diagnosis is not unimportant, since they may be confounded with more serious visual affections. Dr. Stark's paper is a model of sagacious and accurate observation.

Salivary Secretion.—Dr. Wright, of Birmingham, in a series of nineteen papers, published in the *Lancet* during the years 1842–43–44, has given an elaborate and complete account of the chemistry, physiology, and pathology of the saliva. He has treated the subject not less extensively in bibliographical than in experimental and practical detail. As his labours have not been referred to by our predecessors, we shall give a brief summary of the principal pathological conclusions.

He lays great stress upon the existence of ptyaline as a peculiar principle, and upon the presence of it and of sulpho-cyanogen, in healthy saliva. The reaction of the latter is invariably alkaline, except in cases of long fasting, when it occasionally becomes neutral. In non-dyspeptic subjects this quality is increased by the presence of acid or indigestible food. The active purposes which saliva serves in the animal economy, according to Dr. Wright, are to stimulate the stomach to exert a specific action upon the ingesta, and to neutralise undue acidity.

He also states, that by injecting saliva, secreted under the conditions of healthy and energetic functions, into the veins and arteries of animals, symptoms precisely resembling those of hydrophobia follow, and that death takes place in a few days or hours.

* *Edinburgh Medical and Surgical Journal*, October, 1843.

Saliva secreted in languid and feeble states produces little or no effect. No chemical difference between what was active or inert could be detected, nor between the saliva of hydrophobic animals and that of those in health, which leads Dr. Wright to conclude that hydrophobia is not the result of any specific poison, but of the saliva itself, possessing in an undue degree its physiological properties.

These experiments are extremely interesting and singular, and appear to have been cautiously conducted. It has long been a popular opinion that the bite of irritated animals was dangerous, and the preceding facts seem to sanction the impression; but we confess it appears most difficult to understand how, in all cases of laceration by fighting animals, more serious consequences do not ensue, if Dr. Wright's theory be correct. Chemistry may fail to detect a difference which really exists, but that hydrophobic symptoms should follow the injection of healthy saliva, is certainly a most strange and unexpected result.

Dr. Wright enumerates no less than nineteen varieties of morbid salivary secretion, as indicative of local or general disorder. When too abundant, it is an almost constant source of pyrosis and other forms of dyspepsia, which are relieved by sedative-astringent gargles. When the secretion is diminished in quantity, other inconveniences result; and Dr. Wright advises stimulating lotions, with the addition of an acid or alkali as indicated, blistering under the jaws, electric shocks, &c. He agrees with Donné, that gastro-enteritis is invariably accompanied with acid saliva. In every form of sickness, if the reaction is acid, Dr. Wright objects to creosote, and advises carbonated alkalies with prussic acid. Acid saliva becomes alkaline by local depletion or counter-irritation over the epigastrium. He also considers acidity of the saliva to be a most valuable diagnostic sign between rheumatic and nervous pains. In all cases of critical salivation in acute diseases, or when vicarious in character, the impropriety of attempting to arrest the secretion is too evident to need comment. Dr. Wright has the merit of directing attention to a new field of humoral pathology, which in its immediate relations to practical medicine is not undeserving of attention.

Cancrum Oris.—Dr. W. Tomkins,* of Yeovil, has related three cases of cancrum oris, or "water-canker," occurring in adults. In

* *Provincial Medical Journal*, August 5, 1843.

Holland the disease is well known, and attributed to marsh miasmata; and Dr. Hamilton states it to be unknown in high and dry situations. Dr. Tomkins' patients had all been exposed to damp night air, and he found the use of quinine and the free administration of wine productive of the best effects. In England this peculiar affection of the mouth is almost limited to children. Mr. Obre adduces some satisfactory evidence of the value of the cautery.*

Tartar on the Teeth.—There is no end to discovery; and we are told by M. Mandl† that this unsightly and offensive incrustation is nothing but a deposit of the skeletons of dead infusoria agglutinated by dried mucus. He seems to revel in the description of the shape, lively movements, and other qualities of these strangely located vibriones. Mr. Levison, of Birmingham, has informed us that he has often observed alternations between the tartar on the teeth and urinary deposits; a fact more satisfactory and probable than the theory of animalcular origin.

Croup.—An elaborate digest of the remedies employed in croup has been written by Dr. Van Berchem,‡ of Anvers, who has diligently culled his materials from the leading writers of Great Britain, Germany, France, and Italy. It forms a convenient reference for future writers, and proves how much is really old which is daily advanced with every claim to novelty. Among the principal remedies mentioned as valuable, during the past year, we may notice the sulphate of copper, by Dr. Schwabe;§ painting the trachea and larynx with a strong tincture of iodine, by Mr. E. Copeman;‖ and inhaling the steam of vinegar, by Mr. Roods.¶

Laryngismus Stridulus.—Mr. Cooper, of Norwich, relates a case of a child, eighteen months old, where the symptoms quickly yielded to a quarter of a grain of belladonna, three times a day, after having resisted free scarification of the gums, counter-irritation, opium, &c.**

* *Edinburgh Medical and Surgical Journal*, January, 1844.
† *London Medical Gazette*, September 8, 1843.
‡ *Archives de la Medecine Belge*, July, 1843.
§ *London and Edinburgh Monthly Journal*, September, 1843.
‖ *Provincial Medical Journal*, August 12, 1843.
¶ *Lancet*, July 29, 1843.
** *Lancet*, November, 1843.

Œdema Glottidis.—An interesting case is recorded by Dr. Carson, of Liverpool,* with the intention of pointing out the superiority of laryngotomy over tracheotomy, and also to illustrate the serious inconvenience resulting from an unnaturally rapid *expiration,* in consequence of the artificial opening allowing a too easy exit to the air. Dr. Carson attributes a fatal result in some cases to this fact not having been appreciated; and in the instance alluded to, the inconvenience was prevented by the patient or an assistant compressing the tube during the expiratory act. The suggestion is highly practical and ingenious.

DISEASES OF THE NERVOUS SYSTEM.

Insanity.—The great question of mental derangement is one too speculatively interesting, and too practically important, not at all times to prove attractive both to the philosopher and to the physician.

No department of medical investigation affords such cheering evidence of rapid progress and improvement as that which relates to the general management of the insane; and the actual condition of our asylums, and the direction of every effort now making for the cure or alleviation of this the greatest of human ills, evidence a remarkable revolution in the knowledge, and still more in the spirit, of the age.

On the nature and causes of insanity, strict unity of conclusion cannot be expected to prevail; the spiritualist and the materialist will never settle the precise boundaries of their union, our knowledge of mind and matter being ever incomplete, and the conditions of their alliance most imperfectly ascertained.

The tendency of recent research has certainly been to exalt the material at the expense of what is spiritual; to make much of the instrument, and but little of the controlling power; to regard, in short, mental aberrations too exclusively as the expression of organic disturbance.

Much practical good has undoubtedly resulted from the clearer apprehension of the truth, that all mental and moral manifestations are effected through the medium of a special organization, and therefore dependent for their exercise on the health and integrity

* *Provincial Medical Journal,* December 2, 1843.

of the instrument; but, while fully admitting such a dependence, and giving due credit to those who have laboured most successfully for its establishment, we have felt conscious that the agency of mind upon matter was virtually, though not always nominally disregarded; and that the actual increase to our knowledge resulting from cranioscopical examination, was really much less than what was so confidently assumed.

These reflections have been suggested by a perusal of a very interesting essay, by the Rev. John Barlow, "On Man's power over himself to prevent or control Insanity," where perhaps the immateriality of the subject will be considered by many to be too exclusively insisted upon.

The author's main object is to establish a distinction between the vital force, in virtue of which man is an animal, and the intellectual force, by virtue of which he is something more. He justly argues, that as the latter is capable of accelerating, or retarding, or disturbing the vital functions, it must have some power in preventing as well as in producing disease. Insanity, the author maintains, does not consist in any one or more morbid impressions, but on the loss of the controlling power, by which their true nature is perceived, and their indications successfully resisted. No amount of disease of the brain and nervous system necessarily renders the individual insane; but whether he is so or not, depends more on the amount of cultivation of the intellectual force, and the guidance which it has habitually exercised over functional aberrations. The strength of the individual's *will* is, therefore, the true test of his mental soundness, and not the healthy or morbid actings of his nervous structure. The most insignificant delusion is sufficient to excite insanity in some; the most terrible impressions are incapable of doing so in others. Man evidently possesses the antagonist power which, if properly used, can to a great extent set at nought the evils of his bodily organization. Many depend for their sanity on the accident of good health, rather than on the cultivation of their intellect; and the great preventive remedy, therefore, is a sound and moral education. Were the records of institutions to be examined, we believe few instances would be found of those who had been submitted to so arduous and ennobling a process.

Such views, while fully compatible with the application of phrenological discovery, tend to elevate our ideas of the great subject of

mental derangement, to account for many apparent inconsistencies between functional and organic disturbance, and to justify a still greater reliance upon moral treatment. We do not attribute the increasing prevalence of juster impressions so much to the progress of science as to clearer apprehensions of revealed truth. It is the stern standard of moral responsibility which christianity inculcates—enlightening our internal experience—that makes man acquainted with his compound and mysterious nature, and prevents his descent to the seductive levels of a shallow materialism. Increased acquaintance with our physical frame and its regulating laws, is dearly purchased if we are led to question the existence or to underrate the influence of the intelligent and spiritual principle. False and cheerless, however plausible, is that philosophy which reduces man to little better than a machine, which discovers in his grand and glorious attributes nothing beyond the agitations of his nervous structure, and makes crime but an excess of functional activity!

No man can doubt, if deducing his conclusions from the knowledge of his own heart, that his mental aberrations from what is right, are moral not cerebral delinquencies; and if so, why should we shrink from admitting the possibility of morbid manifestations sometimes originating in a perverted spiritual principle, as well as in diseased organization? Such a supposition is consistent with the statements and facts of revelation; with what we know of the daily and hourly operations of our own mind; and will, we feel confident, be increasingly acknowledged in proportion as we ascend to a sounder and more comprehensive philosophy.

This is the age of physical investigation, the era of things that are seen; where the great mysteries of life are by many supposed to be only awaiting the unveiling of microscopic discovery; where organization is expected to become its own interpreter; and thought, mysterious thought, with the unearthly longings and outgoings of an insatiate spirit, are but magnetic changes responsive to the magic influence of mesmeric power! Such, we are aware, are not the views or the feelings of those we have now the privilege of addressing, but of late they have been sufficiently prominent to merit this passing condemnation.

The *classification* of mental diseases has engaged the attention of many eminent observers, and has necessarily varied with the pathology and metaphysics of the investigator. Mr. Barlow has

divided them, and we think correctly, into morbid affections of the nervous system and the brain, and into morbid affections of the intellectual force.

Dr. Johnson, of Shrewsbury, in his meritorious prize dissertation, has adopted a purely practical standard, and classed them into congenital and inflammatory disorders, while Dr. Von Dreer* reduces all possible deviations of the human mind under two principal heads. The first is characterised by the ascendancy of psychological over physiological phenomena; all acts resulting from a voluntary subjection of the former to the latter, including errors, vices, and crimes; whilst the essential character of the second class consists in the involuntary ascendancy of the physiological over the mental power, which is in consequence impeded, misguided, or suppressed. The knowledge of one's self, Dr. Von Dreer justly observes, should form the basis of the researches of the medical psychologist. He who accurately observes and is conscious of the various *transient* mental disturbances induced by external circumstances, and by the physical changes of his organization, will be enabled to trace within himself all those forms of *lasting* mental distraction which constitute mental diseases. There is much truth and interest in the remark.

Some speculations on the immediate exciting *cause* of insanity have been recently hazarded; and Mr. Sheppard, though we think upon very imperfect and insufficient data, ascribes it to some morbid state of the blood.

Dr. Prichard has related a singular case of chronic moral insanity, with the view of pointing out that the principal and fundamental cause of derangement is in many instances to be sought, not in the brain, but in some other region of the body. Whether in such cases there always intervenes a morbid affection of the brain, which last is the immediate antecedent of disturbance of the mental faculties, we are not, says Dr. Prichard, in a position to assert.

Dr. Jacobi, and other German observers, have directed the attention of pathologists to the connections which are often traced between the different manifestations of mental disease and various morbid phenomena discovered after death, in organs subservient to physical life; and they seem to infer that the intellectual power may

* *Medical Times*, February 3, 1844.

be as directly disturbed through the medium of the viscera as by any agency exerted immediately upon the brain.

Facts of this description appear to us nothing more than illustrations of the principle, that the brain may be sympathetically disturbed as well as other organs, but not to invalidate the cerebral origin of insanity. The absence of tangible disorganization is no argument against such a view, for it is highly probable, if not certain, that the cerebral peculiarities productive of insanity are not such as are common to ordinary inflammatory changes. These are often associated, no doubt, but there is something beyond what the scalpel or microscope have yet revealed.

The brain, before all other organs, would be expected to manifest and to suffer from distant sources of irritation, and the early detection of these and their removal, form an important part of their successful treatment. Perhaps the great prominence which has been given to the study of the brain as the organ of the mind, has somewhat diverted the attention of observers from these unquestionable and frequent causes of cerebral irregularities.

A very sensible remark has been made by Mr. George Ross,* in his "Analytical Inquiry into the Nature of the Process of Digestion," relating to the effect of a farinaceous diet in inducing irritability of the brain. "We talk much about nervous debility and nervous irritability," Mr. Ross very justly observes; "but we forget that these are simply overt conditions of a particular structure; that it is this structure that is diseased in its intimate texture; and since it is momentarily undergoing decay and restoration, it must of necessity receive modifications according to the elements present for assimilation." The testimony of various observers as to the good effects resulting from an improved dietary in our public asylums, fully justifies Mr. Ross's remarks.

The statistics of insanity in France have been extensively collated by M. Moreau de Jonnes, who concludes in favour of the gradual increase of the disease. His results in proof of the preponderance of physical over moral causes, have been strongly controverted by M. Brière de Boismont and others, and the question is still arithmetically undecided.

A memoir on "Heredité," as a cause of insanity, with an atlas containing thirty-eight tables, was read on the 1st of April, before

* *Lancet*, March 8, 1844.

the French Academy, by M. Baillarger, who concludes that the disease of the mother is more dangerous than that of the father, not only because it is often hereditary, but also transmitted to a greater number of children; that the girls are oftener affected when the disease is transmitted by the mother, and the boys by the father; that its transmission from the mother to boys is not more frequent than from the father, whereas in girls it is twice as much. The greater frequency of insanity among women is also confirmed by Mr. Webster's elaborate researches, referred to by our predecessor, and is in accordance both with theory and general experience.

Dr. Souden,* of Stockholm, has given an interesting account of an epidemic characterized by morbid religious excitement, most extensively prevalent in the central parts of Sweden, in 1841–42. It evidently belongs to the same class of disturbances so admirably described by Hecker, and is clearly traced by Dr. Souden to a combination of causes, all tending morbidly to rouse and misdirect the religious sentiments, and including in its victims the least regulated and most excitable minds. This is, in fact, the common history of all similar visitations, and the contrast is singularly strong between the wild irregularities of disordered imagination and those earnest periods of religious fervour, when the best and wisest of men have counted their lives cheap in defence of their most holy and sacred faith.

We cannot here forbear quoting the dying evidence of the excellent Dr. John Cheyne, who, in his valuable essays on partial derangement of the mind in supposed connection with religion, says, "I firmly believe that the gospel received simply, never, since it was first preached, produced a single case of insanity; the admission that it has such a tendency ought never to have been made to the enemies of the Cross. We have granted that fanaticism and superstition have caused insanity, as well they may; nay, derangement of the mind may often have been caused by the terrors of the law; but by the gospel—by a knowledge of and trust in Jesus—*never*."

Mr. Burnett, of Alton,† has stated his experience of the great advantages of free depletion during the precursory symptoms, or

* *Dublin Journal of Medical Science*, November, 1843.

† *Provincial Medical Journal*, October 7 and 21, 1843.

immediately after the first outbreak of insanity, both in arresting the disease and averting its after consequences. Experience has taught him to regard hysterical fits in plethoric girls with much more circumspection than formerly. The condition of the cerebellum he considers to be essentially inflammatory, and ought to be treated as such. There are several remarkable cases on record of suicides recovering after copious hæmorrhage from attempted self-destruction, and no doubt many instances occur where Mr. Burnett's practice would admit of useful application; but we doubt if free depletion in the early stages of mental derangement is not to be considered as the exception rather than the rule.

The observations of Dr. E. Sharkey, of Jersey, on the employment of large doses of digitalis in idiopathic epilepsy, in 1841, may be remembered by our readers. The same observer* advises its equally bold use in epileptical mania, a disease which, he says, confers a tolerance of the remedy to an extent which would be dangerous under ordinary circumstances; from a drachm to half an ounce of the tincture constitutes a dose.

Dr. Souden ascribes its failure to its timid administration, which is the cause also assigned by Dr. Holland, when discussing the contradictory statements of the qualities of this most singular medicine.

Three cases of *delirium tremens* are detailed by M. Hérzogen,† where large doses of tartar-emetic proved effectual after opiates had failed.

Some interesting experiments have been made by Professor Otto, to determine the action of different medicines upon the brain. "If the psychological action of medicines," he says, "were better known, medical men might be able to vary their exhibition according to the characters and mental peculiarities of their patients. The treatment of different kinds of monomaniacal derangement, also, might be much improved; and it is not improbable but that even a favourable change might be wrought on certain vicious and perverse dispositions, which unfortunately resist all attempts at reformation, whether in the way of admonition, reproof, or even of correction."‡ Though much less sanguine of results than the Professor, his ingenious speculations are by no means destitute of practical interest.

* *Medical Gazette*, December 8, 1843.

† *Bulletin Medical de Bordeaux.*

‡ *Medico-Chirurgical Review*, April, 1844.

Idiotism.—The cause of the idiot has been warmly and generously espoused by M. Seguin, of the Bicêtre.* His labours have attracted the attention of the government, and his self-devotion has already elicited results of a highly encouraging nature.

Another labourer in the same field is Dr. Guggenbuhl, who has established an institution near Interlachen, in Switzerland, expressly for the benefit of the Cretins, of whom, says Dr. Irving in his interesting pamphlet, not less than eight thousand are completely idiotic, and there are double or treble that number more or less affected.

Such men and such efforts render humanity their debtors, and richly merit our gratitude and esteem; they are among the really great of the earth!

Criminal Responsibility.—A great deal of discussion has taken place on the important and difficult question of the value of the plea of insanity in criminal cases. Perhaps the present tendency leans too much toward the criminal; and the principle that punishment is mainly instituted for the protection of society is in danger of being slighted.

"The certainty of punishment and disgrace," Dr. Mayo justly observes,† "is a motive peculiarly likely to influence minds which, without such apprehension, would be easily led to irregulated acts. There are always individuals on the verge of insane actions, and their conduct materially depends on the treatment experienced by those who commit them. No precise limits between responsibility and irresponsibility can ever be assigned; each case must, after all, be tested by its peculiar circumstances, and the decision entrusted to the common sense of the jury."

Doubtless many a guilty action will be passed on to the surer tribunal of heaven for retribution, and many an action be condemned and punished here for which the sufferer will never be held accountable before the throne of God. It is a fearful, dark uncertainty; but we may hope for increasing light and guidance in proportion as the question is dispassionately discussed. Some very excellent editorial articles have appeared in the *Lancet* and *Medical Gazette*, and the whole subject is very ably treated by Dr. Guy in his recent work on "Forensic Medicine."

* *Annales d'Hygiéne Publique*, Juillet, 1843.

† *Medical Gazette*, December 8 and 15, 1843.

Sleep.—Numerous hypotheses have been suggested to account for the phenomena of sleep. The latest is that of Mr. Playfair,* who refers it to diminished oxygenation of the brain. Various conditions, healthy and morbid, tend to increase or diminish the amount of oxygen in the system; and if the theory be correct, it would both suggest and account for many practical indications when attempting to regulate any excess or deficiency of cerebral activity. That sleep should follow a poisonous excess of oxygen, involves no real contradiction, the necessary consequence being an arrest of nutritive transformation and a retention of carbonic acid. The diminished oxygenation during sleep would account for the development of petechiæ during the night, the process of decay passing into one of putrefaction; and many rational therapeutic appliances might be regulated by ascertaining the excess or deficiency of this all important agent. It would be easy to speculate, but we shall simply submit Mr. Playfair's opinion to the judgment of our readers.

Cerebral Auscultation.—To our transatlantic brethren is fairly due the credit of having first successfully studied cerebral auscultation. It is now about five years ago since Dr. Fisher's interesting observations were published. Dr. Whitney, of Massachusetts, has continued the investigation up to the present moment, and he has proved it to be not destitute of physiological and practical interest.†

It would appear that in all cases where the cerebral circulation is increased, whether from inflammation or temporary congestion, as after hooping cough, a bellows sound, more or less intense, may be detected; and that it may be regarded, in a general way, as indicative of increased vascularity of the head. In inflammatory affections, Dr. Whitney considers it a valuable test of the progress of the case, but the conditions of its existence are too numerous to render it alone diagnostic of any particular affection. When effusion has taken place, the vocal resonance has a trembling, brazen-vibratory character. A peculiar rough-blasting sound was present in a case of apoplexy, from ossification of the arteries. On the whole, the subject of cerebral auscultation is deserving of the practitioner's attention, and promises to multiply our diagnostic

* *Northern Journal of Medicine.*

† *American Journal of the Medical Sciences*, October, 1843.

and therapeutic indications in a class of diseases not unfrequently involved in the deepest obscurity. Our space will only admit of this brief allusion to the subject.

Hysteria.—We have little to record of interest or novelty on the subject of hysteria. In America a great deal of attention has been excited by a case where more than forty spiders were removed from the eye of a young lady.* Many sage explanations have been suggested of so marvellous a phenomenon; but it merely proves that the entire history of hysteria will never be written so long as a fresh patient remains to be examined.

Dr. Greenhow† relates four singular cases of imitative hysteria, characterized by distressing singultus; they at length yielded to mustard emetics, which Dr. Greenhow has found of much service in the treatment of this protean disease.

The ergot of rye is recommended by Dr. Nardo,‡ as very efficacious in all cases characterized by general debility. He gives a scruple of the powder for several days, suspending it for a short time, and again renewing it.

Epilepsy.—An interesting case is related by Dr. Parish,§ of twelve months duration, consequent upon a blow, which yielded to a pea issue, two inches long, over the seat of the injury. It is one of many instances where a small point of local irritation has proved the source of this terrible disease, and exhibits the importance of closely examining every case.

Dr. Parish remarks, that the practice of incising the scalp, as recommended by Drs. Prichard and Wallace, recently illustrated by the latter, has for several years past been employed by Dr. Charles Evans in the treatment of chronic affections of the brain, with very satisfactory results.

Dr. Wakefield Scott‖ has adduced some additional cases confirmative of the value of large doses of digitalis, as advised by Dr. Starkey and others; and M. Semoine states that he has successfully treated three confirmed cases by the following mixture, the value of

* *American Journal of the Medical Sciences*, July, 1842.

† *Medical Gazette*, December 22, 1843.

‡ *Medical Times*, April 13, 1844.

§ *Philadelphia Medical Examiner*, vol. vi., No. 2.

‖ *Provincial Medical and Surgical Journal*, April 3, 1844.

which can only be inferred from the assertion of its efficiency:—Liq. ammon. acet., ℥xij.; Syrup aur., ℥j.; Aquæ dest., ℥ij.; Aquæ lauri ceras. ℥j. Sum. cochl., iij. ampla in die.

Nothing can be more vague and unsatisfactory than the generality of reported cures of epilepsy. Very careful discrimination, and very numerous and long-continued experiments would be necessary to arrive at any trustworthy results. The clearer appreciation of reflected sources of cerebral irritation appears to be the principal advance which modern investigation has made in the diagnosis and management of this most distressing complaint.

In the *convulsions* of children, depending on dentition and other analogous causes, Dr. C. S. Tripler, of America, recommends the use of *mustard* internally, which possesses rapid counter-irritating qualities, and seems to be of use independently of any emetic effect.

Tetanus.—In the treatment of tetanus Mr. M'Coy* states, that the late Mr. Wallace diminished and protracted the intervals of the spasms by passing a bougie into the urethra, smeared with strong mercurial ointment; and in the Westminster Hospital considerable benefit appeared to follow strangury from the use of turpentine. Five grains of the extract of belladonna proved successful in a case of Dr. J. C. Williams;† and Dr. Hutchinson, of Nottingham, relates two more, benefitted by the same means. In the *Bengal Transactions* we find that Mr. Brown succeeded in two severe cases of traumatic tetanus, by a fourth to half a grain of tartar-emetic every quarter or half hour, keeping up nausea until the symptoms had subsided.

Catalepsy.—A very interesting and detailed example of this singular condition is given by Dr. Chowne,‡ which we rejoice did not fall into the hands of the mesmerisers.

Paralysis Agitans.—Three cases§ occurring in children, from eleven to thirteen years of age, have been detailed by Dr. Hennis Green. The condition is very rare in the early periods of life, and the symptoms are very distinct from those so characteristic of chorea.

* *Dublin Medical Press* and *Medical Times*, February 10, 1844.

† *Lancet*, May 18 and 25, 1844.

‡ *Lancet*, August 5, 1843.

§ *Provincial Medical Journal*, February 24, 1844.

There is simply an equal and rapid oscillation of the limbs in the line of flexion and extension.

Chorea.—M. Trousseau* advises the use of strychnine in chorea. He administers it during or after meals, in cautiously increased doses, until convulsive movements take place. These are to be maintained for about eight days, when, by diminishing the dose, a mere muscular stiffness should be kept up for a similar period, and the remedy persevered in for eight or ten days after all irregular movements had ceased. He cites eight cases in proof of his assertions, but we doubt if successes would justify the employment of a remedy so difficult to regulate, especially in circumstances which very seldom resist ordinary and well known treatment. The frequent use of powerful medicines, particularly with children, is unquestionably a tendency which needs no encouragement.

Paralysis in children has not, perhaps, received all the attention it deserves. Dr. West,† in his excellent observations on some of the more important diseases of childhood, has given examples of it occurring congenitally, and also after convulsions, and the still more frequent instances where there is no indication of cerebral disease. Reflex movements, he observes, are seldom noticed, and the prognosis is, for the most part, favourable. Mild purging, stimulating liniments, and tonics, form the basis of rational treatment.

Cerebro-spinal Irritation.—Dr. Holland‡ mentions a remarkable and, he believes, a unique instance of cerebro-spinal disturbance, in a collier, aged 28, who, after suffering from frontal headache with symptoms of digestive derangement, was seized, when in the recumbent posture, with an irresistible impulse to talk, with very rapid and indistinct articulation, incoherent and ridiculous ideas, and frequent repetition of the same words. The patient was perfectly conscious of these peculiarities, but quite unable to prevent them. Soon after this he had, about four times a day, paroxysms of rapid rotation of the head, from shoulder to shoulder,

* *Journal de Medecine de Beau*, Juin et Juillet, 1844.

† *Medical Gazette*, September 13, 1843.

‡ *Edinburgh Medical and Surgical Journal*, January, 1844.

not less than eighty times in a minute, followed by temporary impairment of hearing, a flushed countenance, and slight mental confusion. The patient gradually recovered, and the seat and nature of these very strange symptoms are left in the vague region of conjecture.

Meningitis.—Dr. Kennedy,* in a very practical paper, directs attention to an insidious form of cerebral disease, setting in with the symptoms of mild fever, and running on from ten to fourteen days before any distinct cerebral disturbance is manifested. It occurs principally in females, and between the ages of 15 and 25. He has seen more than thirty cases, and feels justified in concluding, that if at the age of 15 a mild fever begins to show untoward symptoms, it is in all probability owing to the affection of the brain now described; he has met with no exception to this remark. Unless early and vigorously treated, the result is uniformly fatal; and after death, inflammation of the arachnoid at the base of the brain, with effusion into the ventricles, are the appearances observed.

Another form of meningitic disease has been described by Dr. Brockmann.† It takes place in children, from the age of 3 to 14, and he has termed it mesencephalic meningitis. He founds his remarks on fourteen cases, and eight *post-mortem* examinations. The early or congestive stage presents nothing peculiar; but with the inflammatory stage are noticed a backward position of the head, the eye and the intelligence remaining unaffected. The temperature of the occiput is increased, and a peculiar form of convulsion, resembling the effects of fright or of an electric shock, supervenes, at regular intervals of some minutes, only ceasing during sleep. The pulse throughout is regular. Towards the close the convulsions cease, a state of somnolence succeeds, but without delirium or change in the expression of the eye; and at the same time, in all fatal cases, a difficulty of moving the tongue, passing on to complete paralysis, with a greater or less amount of deafness, are characteristically present. Dr. Brockmann advises the ordinary treatment adopted in this country, and speaks highly of the value of carbonate of ammonia, in doses of two or three

* *Dublin Journal of Medical Science*, July, 1843.

† *Archives de la Medecine Belge*, Aout, 1843.

grains every two hours, increased to five or seven grains, and gradually discontinued as the symptoms diminish. Dr. Brockmann is evidently a sound practical observer.

Paralysis.—During the previous year Dr. Budd* had pointed out the tendency to hemiplegia in persons whose constitutions were impregnated with the syphilitic virus. A confirmatory case was afterwards narrated by Dr. Todd; and more recently Mr. Inman, of Liverpool, has published five instances illustrative of Dr. Budd's observations. It is not easy always to determine whether syphilis or mercury is the exciting cause of the paralysis; but that one or the other, or both, tend to induce structural change in the nervous structure, and to impair or destroy its functional power, there can be no reason to doubt.

In all cases of partial paralysis,† the surgeon of the Dreadnought states, that he has detected a line of increased nervous sensibility to touch, at about an inch in extent, exactly where the nerves terminate, whose origin is above the diseased portion of the cord. This is attributed to inflammation set up in the immediate vicinity.

Dr. Oke,‡ of Southampton, has given an ingenious sketch of the relative anatomy of the spinal column, with the view of making each vertebra an indication of the parts in connexion with, in juxta position to, or in a line with it; and thus to make each subservient to practical purposes in the diagnosis and treatment of disease.

Injuries of the Head.—The elaborate and laborious statistical investigations of Drs. Laurie and King§ on injuries of the head having rather a surgical than medical bearing, we shall avoid any analysis of their valuable essay, and content ourselves with recalling their practical conclusions.

There are no means, they believe, of distinguishing the milder forms of injuries of the head from each other when there are no physical signs; and all such cases should be carefully watched, on account of the insidious manner in which dangerous complications set in. No single symptom, nor combination of symptoms,

* *Medical Times*, August 5, 1843.

† *Lancet*, August, 1843.

‡ *Provincial Medical Journal*, February 10, 1844.

§ *London and Edinburgh Monthly Journal*, October and November, 1843.

points out the situation of the injury in the more severe and complicated cases. The symptoms most characteristic of *concussion* are intermittent sensibility, vomiting, absence of paralysis, and calm breathing. The symptoms which indicate *extravasation* or *laceration* of the brain are continuous insensibility, extensive paralysis, stertorous breathing, dissimilarity of the pupils, feeble pulse, and early convulsions.

The treatment recommended is, with few exceptions, antiphlogistic; but the necessity of pursuing rational indications, as to the nature and amount of sanguineous evacuation, and the importance of avoiding the too often indiscriminate practice of active depletion, are very clearly and judiciously stated.

Neuralgia.—The frequency, urgency, and obstinacy of neuralgic complaints render them peculiarly the subject of fresh therapeutic experiments, and from their great diversity of cause and situation, the practitioner cannot be too well acquainted with what others have found successful in their removal. Except as an external remedy, belladonna has been less frequently employed by British practitioners than its acknowledged efficacy in subduing pain would have led one to infer. Its effects upon the nervous system, when contrasted with those of opium, have been very partially recognised, and, with few exceptions, its administration has been grounded upon empirical reasoning.

Dr. Hutchinson,* of Nottingham, has communicated the result of his extensive experience of its curative value in neuralgic affections, and the evidence adduced will probably encourage others to more frequent trials of its therapeutic powers. He considers its primary action as excitant upon the brain, and sedative upon the spinal system, while that of opium is the reverse.

If this observation prove correct, belladonna should always be preferred to opium in convulsive affections induced by direct or reflected irritation of the true spinal system, and avoided in congestive or inflammatory conditions of the brain. The constitutional effects are quickly induced by its external application, or by a quarter of a grain to a grain given internally, three or four times a day. No benefit has been observed until the iris is affected.

* *Lancet*, September 8, and October 14, 1843.

Mr. Flood, of Leeds,* confirms its curative value, but considers its action purely sedative, and therefore adapted to every species of nervous excitement.

The inoculation of solutions of morphine and veratrine, by numerous small punctures with a lancet point, over the affected nerve, appears to have been successfully practised† by Messrs. Lafargue, Jaques, Uytterhoeven, and others, and is at once a rational and convenient application.

M. Rougier,‡ to test the completeness and permanence of the cure by morphine, administers small doses of strychnine. He says, that if the cure is satisfactory, the specific effects of strychnine decrease from the first, notwithstanding an increase in the strength and frequency of the dose; but if not, the reverse. The idea is an ingenious one, but we doubt its accuracy.

The sulphate of strychnine, in doses of one-twelfth of a grain two or three times a day, occasionally combined with sedatives and quinine, is highly recommended by Dr. John Waters; and several Italian physicians unite in praising the valerianate of zinc,§ in doses of one or two grains. Another observer states that he has tried the strong infusion of tobacco in frictions, with complete success in two cases, and a preparation of the extract mixed with cerate, is suggested as a more convenient application.

A dossil of lint steeped in the liquor ammoniæ, and applied to the alveolar border of the palate until an abundant effusion of tears is excited, is said to act very rapidly upon facial pains.‖

Electro-puncture has been strongly advocated by Dr. E. Hermel,¶ who thinks it available in all cases of idiopathic neuralgia, and also useful when paralysis has followed severe pains. This means has before found a zealous defender in Dr. Shuster;** and though his statements may be considered as overstrained, yet there can be no doubt that his favourite remedy has frequently succeeded when other measures have failed.

* *Lancet*, September 23, 1843.

† *Medical Times*, September 9, 1843; *Bouchardat Annuaire de Therapeutique*, 1844; *Comptes rendus*, Novembre 24, 1843.

‡ *L'Experience*, July, 1843.

§ *Bulletino delle Scienze Mediche.*

‖ *Lancet*, August 19, 1843.

¶ *Ann. Med. Psych.*, Mars, 1844.

** *Gazette Medicale*, Janvier, 1843.

Dr. Duclos has written a memoir on the good effects of *pressure*, to a degree that is painful, applied between the angle of the jaw and the mastoid process, in arresting facial neuralgia, nervous headache, and in many cases of epilepsy and hysterical attacks. In occipital neuralgia it is to be applied between the splenii and complexus muscles; and in toothache, on the gum, over a neighbouring sound tooth. Melancholy, says Dr. Duclos, may be dispersed by reiterated compression of the facial nerves. He is, in fact, an enthusiast for pressure; and though he has probably pressed his conclusions beyond their sober limits, a good practical hint may be gleaned from his researches.

In neuralgia of the breast, Dr. Rufz, of Martinique, advises two or three sections of the gland, by a tenotome under the skin, if other means have failed to relieve. Two cases are related in favour of the suggestion.

Sciatica.—The application of a blister, dressed with belladonna ointment, is favourably mentioned by Dr. Hutchinson.

M. Fioraventi* narrates twelve successful cases from the effect of a blister to the heel. After applying a poultice the epidermis must be carefully thinned by a bistouri.

Dr. Petrin's method† consists in using an olive-shaped cautery, at a white heat, between the little toe of the diseased limb and the one next it; his success is reported to be great. Irritating applications to the heel are sometimes employed by empirics, and the practice is worthy of remembrance.

Thirty-one parts of cerate, with eight of belladonna and three of the acetate of morphia, is reported to be a very useful form of neuralgic embrocation.

Cephalalgia.—The association of opium with vascular congestion of the brain, is so general, that its employment against pains within the cranium is either timidly adopted or wholly laid aside.

Pain, M. Despeaux,‡ justly argues, is no evidence of vascular excitement, nor does it necessarily induce it when even continued and severe; when unaccompanied with general febrile disturbance,

* *Annali Universali di Medicina*, November, 1843.

† *Gazette des Hospitaux*, Novembre 25, 1843.

‡ *Annales de Therapeutique et de Toxicologie*, Janvier, 1844.

this symptom is most successfully treated by opium, whether in the head or elsewhere. After premising in some cases a moderate bleeding, M. Despeaux administers a quarter of a grain of morphine every hour until relief is obtained; and since adopting this plan, he says that his success has been most gratifying. We are satisfied that opium is often available against headache where many object to its employment; and when no particular idiosyncracy prevails, we have never seen anything beyond very temporary inconvenience result.

Stammering.—M. Colombat,* of Isére, has devoted much time and attention to the causes and treatment of vocal impediments. He divides stammering into two kinds: the one, a spasmodic condition of the articulating muscles; the other, a tetanic rigidity of the respiratory muscles. In the first variety, individuals speak easily, but with repetition of certain labial letters; while in the other the speech is slow, with evident effort, and the guttural sounds are most affected. Against the first he advises free employment of the zygomatic muscles; and for the tetanic variety, it is important that a deep inspiration should precede utterance, at the same time forcing backward the tongue, so that its point should approach the velum, while the lips are extended and the jaws separated as widely as possible for the pronunciation of syllables.

M. Colombat insists upon great attention to the rythm of sentences, and has invented an instrument termed "Muthonome," to facilitate syllabic exercises. M. Colombat states, that during fifteen years he has restored 782 persons, of whom 643 had no relapse, and that the average of treatment was about a month.

M. Jourdant rather differs from M. Colombat's conclusions, and maintains that stammering is, for the most part, a consequence of too rapid expiration. He advises the patient to perform a natural inspiration, the course of which is to be suddenly arrested; a sentence is then to be begun, slowly pronounced, and continued till it, or a fixed portion of it, is finished. The remaining air is then suffered to escape, and after a short pause the same process is to be recommenced. The medium duration of treatment is about a month, the patient practising two hours daily.

* *French Academy of Medicine*, July 4, 1843.

Mr. Yearsley* candidly regrets that more discrimination did not mark the outset of his own practice.. He now admits the great importance of educational and medical means as accessories to operative measures, and in two ingenious communications asserts, that the basis of every successful method hitherto proposed, involves the rythmical principle.

DISEASES OF THE CHEST.

Diagnosis.—The elaborate essay of Mr. F. Sibson, "On the Changes induced in the Situation and Structure of the Internal Organs, under varying circumstances of Health and Disease, and on the Nature and External Indications of these Changes," published in the last volume of our *Transactions*, forms one of the most original and valuable contributions to physical diagnosis which has been made since the days of Laennec.

The very ingenious and novel method of investigation adopted by the author, ensures an accuracy of result, not otherwise attainable, and he has succeeded in developing a freshness and interest in his subject, which, judging from the multiplicity of labourers in the same field, could not have been anticipated.

Mr. Sibson displays peculiar aptness and sagacity for physical research, and has not only added fresh facts and suggestions of his own, but given additional clearness and precision to phenomena with which we were previously acquainted.

The absence of distinct ideas of the natural boundaries of our organs, has been one great source of the imperfect application of physical diagnosis, and we are aware of no source of information better calculated to remedy this defect, than the one Mr. Sibson has provided.

We would suggest the propriety of giving to his essay a wider and detached circulation, somewhat condensing the detail, and adapting the arrangement to practical convenience. The diagrams would be more easily studied if simply numbered and grouped, and their intention and value be better appreciated if each organ were uniformly outlined by a particular colour. The author has laboured long, but not in vain; and from the value of his interspersed pathological observations, we feel confident that the present

* *Lancet*, November 24, and December 2, 1843.

is not the only contribution for which the profession will have to thank him.

We may have occasion to refer to some of the more interesting results, but the essay is too recently in the hands of the members to require on our part more than this very general allusion to its merits.

M. Trousseau, in one of his clinical lectures, shows the importance of distinguishing the phenomena of respiration in children, from birth to the age of two and a-half to three years, and the subsequent period. During early life the thorax is imperfectly developed; percussion yields a moderately clear sound, and the respiratory murmur is feeble. Puerile respiration is not developed till the voluntary powers are active. If the child is lying on the face, percussion is posteriorly dull: and if the child is irritated, the inspirations being rapid and the expirations slow, the former assume a more bronchial character, and the latter are inaudible.*

Phthisis.—Notwithstanding all that has been written on the ever important subject of tubercular consumption—though remarkably acquainted with the minutiæ of its diagnosis, capable of detecting its earlier stages of development, and of tracing its progress with a precision greater, perhaps, than we can boast for almost any other internal disease—yet opinions are still divided upon the etiology and nature of tubercular deposit, and our treatment remains as ineffectual and empirical as ever. Contrasting what we know with what we accomplish, no better instance can be cited of the inadequacy of mere detail to generate practical results; we must await the impetus of some master spirit, who shall gather up the rich but disunited fragments, unfold their harmonies, and disclose their bonds of union; or perhaps we are destined only to analyse, but never to arrest this fearful process of human destruction. At all events, the time of consummation to our wishes has not yet arrived; and though many have laboured to promote its advent, the indications of its approach are faint indeed.

Such, we are aware, is not the conviction of even some of the most rational and truthful observers; and could we believe the confident assertions of the less scrupulous, the curability of phthisis, the absorption of tubercles, are facts of every day occurrence; but

* *Gazette des Hôpitaux*, No. 7.

we confess ourselves among the number of those who have to learn that a single instance of pulmonary consumption has been indebted to the materia medica for its cure.

We shall, however, limit our remarks to the materials which have presented themselves to our notice during the past year.

Mr. Sibson, in the memoir alluded to, maintains that tubercle, whether in a miliary, granular, or diffused form, is seated in every component texture of the lung. He considers inflammation to be concerned in the origin of all tubercles; and that the essential and first morbid change is the formation of peculiar cells. There is a modification in the cell-life of the walls of the capillaries; wherever the capillaries are met with, there may the morbid change exist; in the walls of the pulmonary artery, and in the pleura, the tubercles are formed with the aid of new or produced capillaries, and a great part of the structure, or as it were the skeleton, the framework of these tubercles, consists of the walls of the new and obstructed capillaries, often accompanied, even in the most advanced stages, by some blood-carrying vessels. Upon, within, and interstitial to this framework of dried-up new capillary walls, is deposited the peculiar structure of tubercles. He institutes an ingenious analogy between pneumonia and phthisis, and directs attention to minute white granules, which are scattered all over and in every texture of the affected part of the lung, consisting of a cell with diaphanous walls, varying in size from the 1000th to the 100th part of an inch, without nucleus, but itself sprinkled with very minute secondary cells, which are also observable on the surface of the tubercles themselves. Mr. Sibson offers no explanation of their nature.

Dr. Lebert asserts* that the constant microscopical elements of tubercles are granular particles, an inter-globular hyaline substance, and the corpuscles or globules peculiar to tubercles, of an irregular angular shape, not nucleated, containing granules, and imbedded in a yellowish and somewhat opaline substance. The grey semi-transparent granulations are not always the origin of, and are often distinct from, the miliary tubercles; and the microscope proves, he thinks, that the former are not caused by inflammation.

The researches of Schroeder Van der Kolk,† on the changes

* *Académie des Sciences*, Mars, 4, 1844.

† *British and Foreign Medical Review*, October, 1843.

induced in the vascular conditions of the lungs from tuberculous deposit, are confirmed by Mr. Sibson's observations, and are not without practical and theoretical interest. He ascertained that the branches of the pulmonary artery cease to be permeable at a short distance from tubercles or grey granulations, and that where the masses are inconsiderable, they are surrounded by a sort of investment, presenting not a single vascular ramification. This condition is only temporary; new vessels are developed, which in process of time communicate either with the bronchial arteries or with those of the walls of the thorax, by means of vessels originating in the pleural false membrane. This rete spreads with the disease, and more or less replaces the system of the pulmonary artery. We have, therefore, aortic blood flowing through a certain portion of the lungs instead of venous—a condition necessarily modifying the circulating fluid of phthisical subjects. We may suppose it to hasten the rapid disorganization of the lung, and induce that hectic reaction which is so characteristic of many cases of intense tubercular disease. It may also explain, in some degree, the benefit resulting from remedies possessing a strong affinity for oxygen, though difficult to reconcile with the advantages said to have been derived from the inhalation of this gas.

Another circumstance that may materially modify the oxygenation of the system, is the fatty degeneration of the liver. The elimination of carbon must be proportionately obstructed, and this element accumulate in the blood, where its injurious influence must be as fatal as its production is rapid. We are not aware whether the symptoms and fatality of phthisis have ever been analysed in relation to the presence or absence of fatty degeneration of the liver, but the investigation presents many points of physiological and pathological interest. The fact of many cases of tubercular disease not leading to any change of the structure of the liver, must probably depend on increased oxygenating power, or may originate in a too active decomposition of the tissues as well as in an excess of the non-nitrogenized elements of food; but in either case the utility of animal diet, steel medicines, and free exposure to the air, recommended by both ancients and moderns, can be rationally estimated and explained.

From an able but anonymous article, published in the *Medical Times*, of the 30th of March, on the pathology of phthisis, we

extract the following summary of the writer's views, believing them to express the prevalent modern opinion of the disease:—If the inflammation, he says, be of a scrofulous, asthenic character, or the nutrition of low vitality, the nucleated particles, or exudation corpuscles as they are termed when the product of inflammatory effusion, may not have the force of transforming themselves into complete pus globules and fibre, or of acquiring the vital organizing force of the tissues. Under these circumstances, the exuded fibrinous matter loses its power of evolving fresh organic germs; the primitive cells themselves, having no formative force, tend only to degenerate; their nuclei disappear; the body of the corpuscle gets broken up and destroyed, and the whole becomes a confused granular mass—*tubercle*. Tubercular development may thus be regarded as caused by a degraded condition of the plastic material, exuded as a consequence of modified forms of inflammation or of nutrition, and by a deficiency in the assimilating power of the tissues.

This view of the subject is interesting, as it serves to demonstrate, that different processes giving rise, when fully established, to very different products, may, when both are impressed by a morbid agency, produce similar morbid results. We can also conceive, how tubercle may follow a local deterioration, without the general cachexia. An organ may be lowered in its vital power from accidental causes, and its nutritive action be perverted, while on the other hand, the general health may undergo an analogous deterioration, and a temporary consumption be the result. We shall not, however, attempt to speculate any farther upon what we still feel to be a *terra incognita*—a region full of darkness and apparent contradictions.

The *Causes* of phthisis have been anew investigated by M. Fourcault,* who ascribes consumption, and chronic diseases generally, to disturbances of the cutaneous functions by atmospheric variations. Humidity is with him the great element of chronic disease, which prevails in proportion as the locality is low and excluded from the free influence of the wind—a statement singularly in contrast to the testimony of practitioners in aguish districts, to which we shall shortly allude.

Two admirable papers have been published in the *Statistical Journal*, by Professor Guy, with the view of determining the

* *Académie des Sciences*, Juin 27, 1843.

influence of different employments upon health, and, in particular, upon pulmonary consumption. His results are confirmatory of those established by the best observers, and the practical conclusion is, that those persons who have an hereditary tendency to consumption should make choice of occupations which are carried on in the open air. If they are obliged to choose some in-door employment, it should be one requiring strong exercise, while they should carefully avoid exposure to dust and intemperance.

Locality of Phthisis.—Whatever the explanation, the fact of phthisis being very unequally present in different localities, and in some almost unknown, cannot be questioned, and to ascertain its relative prevalence in particular districts, is a point of considerable practical importance.

Some very striking evidence* has been furnished by Mr. Allen, of Guelph, (an upland region of Upper Canada,) who, though an old practitioner, asserts that he had never seen a case of consumption or scrofula in the district, which is remarkable for its bracing character and general salubrity. Dr. Thompson states,† that phthisical patients who have migrated to any of our Polynesian colonies, and adopted the life of a squatter, or gone into the bush, have invariably got better, and continued to improve, while they remained in the interior. The mode of living in the bush is one of the most simple: plain-dressed meat, with farinaceous diet, the best of vegetables, good milk and butter, no stimulants, mild balmy air, an equable temperature, and agricultural occupations, with a considerable share of horse exercise. Shooting and hunting the kangaroos and native dog, form the amusement of the squatter in his leisure hours. Dr. Thompson has certainly outlined the beau ideal of an anti-tubercular existence.

In the Report of the British Dispensary of Syria, by Dr. Kerns, the winter climate of Beyrout is said to be unsurpassed in its adaptation to pulmonary complaints.

The influence of sea air upon phthisis has been investigated by Dr. Vehrneghe, of Ostend, who says, that sailors and fishermen seldom suffer; that consumption forms only one twenty-fifth of the mortality at Ostend; and that, out of 240 patients in the Civil Hospital, only three were phthisical.

* *Lancet*, September 8, 1843.

† *Medical Times*, February 17, 1844.

Dr. George Harward* has laboriously analysed the whole number of deaths for thirty years, in the cities of Boston, New York, and Philadelphia, with a view of ascertaining whether consumption was increasing or diminishing. The most interesting result of his researches, is the fact of the great *decrease* of deaths from consumption in these cities. This has been the case in all, but especially in Boston. From a diminution in the deaths may be fairly inferred a decrease in the disease, and the probable causes are all those influences which are included in the progress of civilisation.

Antagonism of Phthisis and Ague.—We are indebted to Dr. Wells† for the first investigations on the comparative prevalence of consumption and intermittent fever, and, after extensive inquiry, his conclusions are in favour of the absence or rarity of phthisis in districts where ague prevails. The subject has continued to attract the attention of many eminent observers from time to time, but we are not aware of its ever having been so very generally discussed as in France and Italy during the past year. It is quite impossible to give a digest of the numerous statements which have been elicited from various quarters; to do so, would require a very accurate and lengthened analysis, and occupy a volume, instead of a short section in an Address. We can only observe, that sufficient evidence has been adduced to prove, that in many malarious districts phthisis is almost unknown, while in others it would appear that its frequency was by no means diminished. The preponderance of evidence is certainly in favour of the rarity of consumption where intermittent fever prevails, but there are too many and too striking exceptions to this fact, to allow of our concluding that these diseases are in any respect to be regarded as incompatible with each other.

The general mildness of many marshy countries, the absence of sudden changes of temperature, the warm clothing adopted by the inhabitants of malarious districts, and the fact, that generations disappear during childhood under such unfavourable influences, are all circumstances to be taken into account, and are, to a certain extent, influential on the prevalence or infrequency of phthisis. It is also probable that the constitutionally feeble—those most

* *New England Quarterly Journal of Medicine and Surgery*, January, 1844.

† *Transactions of the Medico-Chirurgical Society*, 1812, p. 471.

phthisically predisposed—would be the earliest and severest sufferers from malarious impregnation; and it is quite possible that a constitution so enfeebled and modified might not be susceptible of tubercular formation, while the majority would be cut off before the period of life most exposed to tubercular development, had arrived.

The fact also of other non-malarious districts being equally exempt from consumption, points out the extreme uncertainty of conclusions based upon such a conjunction, and the high probability is, that the local absence of phthisis, where intermittent fever prevails, is dependent upon other causes, and that the idea of these diseases being antagonistic of each other, is founded upon false reasoning and superficial observation. There is, however, much interest attached to the investigation; it will lead to the ascertaining of the relative prevalence of consumption in particular localities, and furnish many valuable data for renewed investigation as to its causes. The French Government is still pursuing the inquiry; and to ascertain where consumption is endemically infrequent, is a point of such practical importance, that we hope our own and other countries will assist in its determination.

According to Escherich,* a Bavarian physician, bronchocele is incompatible with pulmonary tubercles, and he thinks its cure injudicious in subjects who are phthisically predisposed. Similar statements are made by practitioners in the mountainous regions of Styria and the Tyrol, and the remark is not undeserving of attention.

Dr. R. Boyd has given a digest of his necroscopical researches on phthisis, during three years, in the Marylebone Infirmary, and we shall briefly allude to some of his least generally admitted conclusions. Out of 206 cases, 132 were males and 74 were females. No case occurred between the ages of eleven and seventeen: the period most obnoxious to phthisis was from thirty to fifty; and the period most exempt, under twenty, in both sexes.

Dr. Boyd weighed the *viscera* of consumptive patients, and found them in both sexes *above the average*, while the weight of the *body* of the male adult was nearly one-third *less* than that of non-consumptive individuals. The mean height of consumptive males was nearly four inches above the general average; and of

* *Lancet*, August 19, 1843.

females, one inch and three-quarters. Dr. Boyd asks, whether the rule laid down by Mr. Hutchinson, that persons in proportion to their weight expire a greater quantity of air, affords any explanation?

The natural or spontaneous cure of phthisis is always a subject of considerable interest. It has found a zealous investigator in M. Ernest Boudet,* who concludes that tubercular degeneration of the lungs and of the bronchial glands, is much more frequently susceptible of a favourable termination than has been usually supposed. Out of 833 infants, from a day to a year old, tubercular deposit only existed in 1 in 64; from one to two years, in 1 in 12; from two to fifteen years, in three-fourths; and from fifteen to sixty-six, in 6 out of 7—rendering their presence during this long period of life the rule, and their absence the exception. No attempt at cure was noticed earlier than the end of the third year, and the frequency of reparative effort increased with age. From fifteen to sixty-six there were traces of cure in 97 cases out of 116, and in 61 out of the 97 there was no evidence of any recent deposit; the disease was, in fact, completely and probably definitively arrested. Out of 197 cases, where cavities existed, in 10 they were cicatrised without any trace of recent disease, and in 8 there was partial cicatrisation with recent deposit. He has since met with fourteen other instances where cavities had evidently formerly existed, and similar statements are made by M. Fournet.

M. Sandras also has collected seven observations illustrative of the cure of phthisis in the third stage. This observer points out the frequency of sudden death in cases where miliary tubercles are extensively distributed through the lung. This form of phthisis is of considerable practical interest, and not unfrequently mistaken. The quick and shallow breathing, the evident though not urgent dyspnœa, the livid and anxious countenance, the rapid thready pulse, the puffed and palid skin, the diffused bronchitic râles, are symptoms which, once seen and explained by necroscopic examination, will never be forgotten. The points most worthy of attention from M. Boudet's researches, are the great prevalence of tubercular formation; its frequent partiality and cure; and the increasing tendency to reparative effort as the individual advances in life.

Treatment.—Cod's liver oil has been extensively used by several observers in the treatment of this disease. Dr. A. Burgraëve

* *Revue Medicale*, Septembre, 1843.

has made an interesting and favourable report to the Belgian Academy on the therapeutic employment of this and other fatty substances. He confirms its value in phthisis, chronic rheumatism, rickets, and in all cases characterised by deficient action of the system, and asserts that no article of materia medica has stronger evidence in its favour. Its first effect is to augment the animal heat; and secondly, to increase the coloured globules of the blood. It acts, he thinks, by furnishing carbon to the system, and rendering respiration more active. Its peculiar advantages reside in its quicker digestion than other oils, and in its not exciting diarrhœa. Though very offensive at first, it appears afterwards to be less distressing than other analogous preparations. The whole question is submitted to the renewed examination of the Academy. Mr. Chalk, of Margate, speaks decidedly as to its value in strumous and rheumatic cases, remarking that it increases the nutrition, and restores a healthy complexion; and in a sensible pamphlet by Dr. Pereyra, of Bordeaux,* it is strongly recommended in phthisis, combining its use with nourishing diet and an issue or seton in the arm or over the seat of the disease. Many cases, he says, are arrested, if not cured, by these means. The editor of the *Medico-Chirurgical Review* suggests, whether a combination of iodine with copaiba might not be advantageously substituted? Another observer, Dr. Bauer, of Tübingen,† for our encouragement, asserts that the action of cod-liver oil differs in nowise from that of other fatty oils, and that he has derived similar results from their use; he utterly repudiates their internal administration. We hear of the glorious uncertainty of the law, but we feel the distressing uncertainty of medicine. The disgusting nature of the remedy is no small objection to its use, and we anticipate its gradually sinking into its former obscurity.

After the statements of Dr. John Hastings, the long-sought-for remedy might have been considered as at last discovered. But, alas! naphtha, like its shadowy predecessors, seems destined to equal if not to a more speedy oblivion. Two or three faint testimonies in its favour‡ have been recorded during the past year, but the evidence of its entire unworthiness to merit the praises so injudi-

* *Medico-Chirurgical Review*, October, 1843.

† *Gazette des Hôpitaux*.

‡ *Lancet*, November 24; *Provincial Medical Journal*, December 23, and January 20; *Medical Gazette*, February 23, 1844.

ciously bestowed, is too general and decided to admit of reasonable doubt. Naphtha may, and probably has its uses, but as a *cure* for consumption, it is utterly to be discarded. We have ourselves rather extensively tried it, and have never derived a single advantage from its use.

Dr. Maddock strongly re-asserts the value of remedies in the form of inhalation, and has no doubt of the advantages of chlorine and iodine, so used. His volume, though far too popular in style and intention, displays a practical acquaintance with disease. On no point can more opposite testimony be adduced than on the value of inhalation in phthisis, but we think this mode of administering medicine may not yet have received the amount of trial and attention which it really deserves.

Dr. Black,* in his Report of the Manchester Union Hospital, states that he has administered the iodide of iron, to the extent of ten to sixteen grains daily, in advanced cases of phthisis, and is satisfied of its general tonic effect upon the system.

"So many persons," says Dr. Marshall Hall,† "presenting dullness of sound on percussion, and no doubtful pectoriloquy under the clavicle, hæmoptysis and disposition to chills, heats, and early morning perspirations, have been benefitted and restored to apparent health by the use of the alcoholic lotion, that I consider it one of the most important remedies in this disease which we possess. A piece of soft linen, the size of a very large sheet of writing paper, folded six times, is to be applied beneath the clavicles, and moistened every five minutes, during all waking hours, with a lotion of one part of pure alcohol with three parts of water."

About 5000 persons are employed in the manufacture of tobacco in France. M. Simeon has again directed attention to their remarkable exemption from tubercular disease.‡

Grinder's Asthma.—Doctor, now Sir Arnold Knight, was the first (1830) who strongly directed the attention of the profession to the peculiar form of pulmonary affection attacking the dry grinders of Sheffield. Dr. Holland, in his recent admirable con-

* *Provincial Medical Journal*, February 24, 1844.

† *Lancet*, April 20, 1844.

‡ *Annales d'Hygiéne Publique*, Octobre, 1843.

tribution to the statistics of this town, has almost exhausted the subject, distinctly pointing out the cause and the remedy. Though, unquestionably, hastening and determining the development of pulmonary tubercles in those predisposed, the disease is not to be confounded with phthisis, but is clearly depending on the inhalation of calcareous and metallic particles, and presents a pathology peculiarly its own. The fact of the apex of the lungs principally suffering can only be accounted for on mechanical principles, which are probably equally applicable to the deposit of tubercle. It seems clear, from Dr. Holland's statements, that a properly adjusted funnel for removing all dust, if generally used, would cause the disease to disappear.

Dr. Favell* has also investigated the pathology of this affection, and promises to publish the result of his inquiries.

An anonymous writer† states that he has enabled asthmatic persons to work comfortably in the most dusty atmospheres by simply covering the nose and mouth with a double fold of silk handkerchief.

Asthma.—Dr. Branson‡ recognises in spasmodic asthma a manifestation of gout in an irritable system, and states that he has prescribed colchicum with manifest advantage. Drs. Forbes and Holland both mention gout as an occasional predisposing cause of asthma.

Burning, or letting the patient smoke paper saturated in a solution of the nitrate of potass, has been found by Dr. Frisi, of Milan, quickly effective, in allaying the asthmatic paroxysm.

Dr. Ducros professes to cure asthma by daily exciting the pharyngeal plexus by an ammoniacal preparation.

Dr. Turner,§ of New York, has given some very interesting details of his own case, which is highly illustrative of the peculiar influence of ipecacuanha, whether inhaled or taken internally, in inducing in particular individuals, and in the smallest doses, the asthmatic paroxysm. Four similar instances are referred to, in two of which it was the only exciting cause; and similar cases, as

* *Provincial Medical Journal*, August 19, 1843.

† *Medical Gazette*, October, 1843.

‡ *Provincial Medical and Surgical Journal*, June 4, 1844.

§ *Boston Medical Journal.*

is well known, have been detailed by different observers. The specific effect of ipecacuanha, as an emetic, has not been before distinctly pointed out, and it becomes a practical question, whether this drug should ever be employed when asthmatic symptoms are to be combated.

The catarrhus æstivus, or summer asthma, first clearly described by Dr. Bostock, may also be dependent on some special vegetable effluvium; but Dr. J. W. King,* himself a sufferer, ascribes it to seasonal changes of temperature acting upon a system with deficient nutrition, and says that it is best relieved by warm clothing, tonics, and sea air. He ascribes the failure in treatment to want of attention to temperature; but we confess, that after perusing the statements of Drs. Bostock, Elliotson, and Gordon, we think the reasons for considering it dependent upon a specific cause greatly preponderate.

Pneumonia.—We have already referred to the peculiarities of pneumonia in old people, as pointed out by Dr. Beau, and shall now briefly direct attention to a valuable communication, by Dr. Addison, on the pathology, diagnosis, and varieties of this disease, published in *Guy's Hospital Reports* for October, 1843.

Dr. Addison combats the opinion that pneumonia, except in the aged and cachectic, is always attended by its characteristic symptoms. He thinks the simple pneumonia of Laennec and others, to be really a broncho-pneumonia, and that a truly simple form of the disease is not unfrequently met with in young persons, and in good constitutions, unattended by either cough, expectoration, or pain, or, at least, such a degree of either as to attract particular attention.

Dr. Addison doubts the existence of any intercellular tissue in the lungs, and thinks the seat of the disease to be demonstratively in the cells, which present the character of areolar or serous, rather than of mucous membrane.

Much time, we think, has been wasted in attempting to classify the tissue of the pulmonary cells: regarding it as *sui generis*, would save much useless discussion, and approximate nearest to the truth.

We may here observe, that Mr. Sibson considers the pulmonary artery, the bronchial tubes, and the pleura, to be the seat of

* *Medical Gazette*, August 4, 1843.

pneumonic inflammation. The capillaries, he says, are quickly and uniformly enlarged, their obstruction early and complete, and a new capillary growth rapidly succeeds.

Some excellent observations are made by Dr. Addison upon the influence of the constitutional condition on the character and changes of inflammatory deposits. Tubercle, he considers to be nothing more than a strumous and inflammatory degeneration of the pulmonic tissue. The fact of its almost invariably being first deposited in the apex of the lungs Dr. Addison meets by the singular statement, that in individuals who have died of disease not affecting the chest, indications of partial inflammation are much more frequent towards the apex than towards the base. Is not this, we would ask, an assertion expressive of the wants of Dr. Addison's hypothesis, rather than the result of unbiassed observation? Our space will not permit a more detailed allusion to this much agitated question.

Some sensible observations on the importance of observing the influence of seasons upon disease have been made by Dr. Henry Bennet,* who relates the curious fact, that between the 1st of February and the 15th of April, 360 patients were received into M. Gendrin's wards, and of these 45 were affected with pleuro-pneumonia. In *every case* the region attacked was the anterior and middle portion of the right axillary region, as far as the nipple. The general symptoms were rather those of influenza than pneumonia; and it may be asked, whether the viscera as well as the skin may not be susceptible of special forms and limitations of morbid action?

Gangrene of the Lung.—Some elaborate researches on the nature and treatment of gangrene of the lungs have been instituted by Dr. Ernest Boudet.† He founds his conclusions upon the examination of 1422 patients, and 506 necroscopical inspections. Pulmonary gangrene he finds to be more common in infancy than at any other period of life. At this age it is rarely limited to the lungs, and cannot be regarded as a merely local condition. The lower lobes are principally affected, and it appears to extend by contiguity, without regard to tissue, and is not materially influenced by

* *Lancet*, September 23, 1843.

† *Archives Générales de Medecine*, Septembre, 1844.

tubercular or inflammatory complications. The exciting cause is an alteration of the blood, characterised by diminished fibrine and an excess of alkalinity. This is usually consecutive to eruptive fevers, and the practical indications would be to invigorate the powers of the system, and to administer the mineral acids.

Malignant Disease of the Lung.—Two cases have been described by Mr. Vickers and Dr. George Burrows.* In addition to the diagnostic signs so ably pointed out by Dr. Stokes, Dr. C. Williams has noticed spasmodic difficulty of breathing to be more connected with malignant disease than any other affection of the lungs, except purely spasmodic asthma or substernal aneurism.

Bronchitis.—Dr. Andrews, of Teddington, strongly recommends the use of alum in acute as well as in chronic bronchitis. His usual formula is forty-eight grains in five ounces of half-boiling water, with half an ounce of syrup. The dose varies from half a drachm to half an ounce three or four times a day, according to the age of the patient.

Foreign Bodies in the Air Passages.—A well-known recent circumstance has directed particular attention to this accident. Mr. J. E. Erichsen has repeated the experiments of M. Majendie, and advises opening the trachea, for the double purpose of preventing asphyxia and presenting a less sensitive outlet. By opening the trachea spasmodic movements are not prevented, but their danger is obviated, and other evident advantages obtained.

Hæmoptysis.—Hæmoptysis in children is comparatively rare. Out of 118 cases of consumption observed consecutively by Dr. Green,† it occurred in five, while in 70 cases recorded at different periods, it did not happen once. Epistaxis is, however, frequently present in young phthisical subjects, and Dr. Green suggests that the one hæmorrhage may replace the other. With his usual diligence he has recorded eight fatal cases of pulmonary hæmorrhage in children between the ages of nine and fourteen. It is possible that in some instances the blood is swallowed and escapes detection; but this could only happen when the amount was small.

* *Medical Times*, February 24, 1844.

† *Provincial Medical Journal*, September 16, 1843.

Pleurisy.—Dr. R. L. M'Donnell,* in a very able and interesting paper, has directed attention to some points in the diagnosis of empyema, which have not sufficiently attracted the attention of observers. He details three cases of empyema complicated with external pulsating tumours, between which and aneurisms or encephaloid disease, he carefully determines the distinctive diagnostic signs. From the former they are principally to be distinguished by the history of the case, by the extent of the dulness, by the pulsation being limited to the external tumour, by the absence of the bellows murmur and thrill, and also by the extent and nature of the fluctuation.

The resemblance to encephaloid disease is still greater, but the peculiar expectoration is wanting; there is no persistent bronchitis, no evidence of pressure, no œdema of the affected side, a pleural limitation of the dulness, and the absence of any general or local indications of malignant disease.

The fact, some years since pointed out by Dr. Greene, of many cases of empyema being cured by copious purulent expectoration, without perforation of the lung, is fully confirmed by Dr. M'Donnell's additional researches. When eliminated from the tubes, the quantity is not at any moment extreme and threatening alarming suffocation, as in the case of perforation; nor is the pus so pure or of so offensive a character as when directly passing from the pleura. The absence, also, of the signs of pneumothorax would be strongly in favour of its tubular origin. We have recently met with an example of this mode of cure, and in addition, there was a remarkable muco-purulent discharge from the bowels.

Dr. Schönlein remarks, in his admirable lectures,† that while he has in some instances observed discharges of serum or pus from the intestines, in general the morbid secretion passes off with the urine, which indicates by its changes the condition of the thoracic effusion. When the urinary secretion is purulent, hectic fever precedes it in the evening; the discharge usually follows about midnight, and ceases towards the morning. The possibility of such facts should be constantly present to the mind of every practitioner.

* *Dublin Journal of Medical Science*, March, 1844.

† *Lancet*, April 13, 1844.

The healthy lung in empyema, Dr. M'Donnell observes, is peculiarly susceptible of bronchitic inflammation; a congestive condition of the tubes is also very generally present, and only revealed by physical examination.

The effect of effusion into the *left* side in displacing the liver is pointed out, and the observer's attention particularly directed to the great alterations occurring in the volume of this viscus, in consequence of the pulmonary obstruction; alterations which may lead to the diagnosis of hepatic displacement, when the organ is simply enlarged, and which at all times exaggerate the apparent change of situation.

Dr. R. Chambers,* of Colchester, mentions a sensation of "gentle gurgling" conveyed to the hand, and indicating the rolling or displacement of a fluid, as an invariable attendant upon pleuritic effusion, and therefore not without diagnostic value.

Dr. Hughes has published a very meritorious and laboured essay upon pneumo-thorax; but as it contains no information of a strictly novel character, we cannot do more than refer to it.

The indications to be drawn from the nature of the effused fluid have been studied by Dr. Krause. He divides it into purulent, sero-purulent, and sanguineous, and found that out of 162 cases it was purulent in 100, of which 50 were cured; sero-purulent in 41, of which 18 were cured; and sero-sanguineous in 20, of which only 2 recovered. These results are of considerable practical interest, and should exert a due share of influence when agitating the propriety of operative interference. The nearer the fluid approaches to a healthy purulent character, the greater the probability of cure.

Considerable discussion has taken place in the Medico-Chirurgical Society, on the curative value of paracentesis thoracis. It originated from an excellent essay by Dr. Hamilton Rowe, who advocates the expediency of an operation at an earlier period than is usually recommended, removing the whole of the fluid, and immediately closing the aperture. Out of 24 cases in his own practice, 18 recovered and 6 died. In 9, the effusion was purulent and only 1 died; 13 were cases of inflammatory hydrothorax, of which 4 died, and 1 was a consequence of cardiac disease. Dr. Theophilus Thomson also cites a successful case in a child, aged six years, where

* *Provincial Medical Journal*, January 27, 1844.

the secretion was purulent—results all in accordance with the facts collated by Dr. Krause. Dr. Rowe proposes to operate in all cases which resist ordinary treatment more than three weeks.

The general experience of the Society was very much less favourable to the success of operation than that of Dr. Rowe; but as the statements of the latter are based upon cases where paracentesis was early performed, and the opposing facts included instances of a very different character, no accurate conclusions can be deduced.

Assuming, however, the most favourable results, we doubt whether a mortality of one in four can be considered as warranting an early recourse to the operation, believing that in ordinary circumstances a larger proportion would recover. In our own experience we cannot recall an instance of death resulting from simple pleuritis; and unless the patient had been neglected at the commencement, or urgent dyspnœa was to be relieved, we should be inclined to trust to ordinary treatment for a much longer period than that named by Dr. Rowe.

Dr G. H. Barlow,* in his interesting "Gulstonian Lectures," has sagaciously examined the effects dependent on the want of proportionate development between the lungs, the liver, and the two sides of the heart. There is much good service done to practical as well as to theoretical medicine by every writer who clearly points out the co-ordination of phenomena, which are too apt to be regarded as accidental and distinct. The imperfect functional condition of the tricuspid valves, for example, in cases of obstructed pulmonary circulation from deficient development of the lungs—an evident means of averting serious mischief, as pointed out by Dr. W. King—would be naturally described as the principal lesion and the nature of the case wholly mistaken, if satisfied with superficial observation. Many morbid conditions have been obscured from undue importance being attached to the more prominent *post-mortem* appearances, and a just diagnosis apparently falsified from the want of correct and consecutive reasoning.

Dr. J. Aldridge† has adverted to the importance of ascertaining whether death has taken place by the brain, heart, or lungs, when

* *Medical Gazette*, January 5, 1844.

† *Dublin Journal of Medical Science*, January, 1844.

estimating *post-mortem* appearances. When the right side of the heart and the lungs are full of blood, the liver is congested, while the left side of the heart, the brain, and the kidneys, are ex-sanguineous. On the other hand, when the left side of the heart, and the arteries at the base of the brain contain blood, the kidneys bleed freely when incised, while the lungs and liver are comparatively bloodless. There are, of course, many intermediate degrees, but the effect of different modes of death on the distribution of the blood should never be forgotten by the pathologist.

Heart.—The state of the heart and large vessels in old people has been investigated by M. Neucourt,* of the Salpetrière, who has carefully examined the circulating organs of twenty-eight females, from 60 to 90 years of age, who had not presented during life any evidences of cardiac disease. From these too limited materials he concludes, that the size of the organ rather increases than diminishes with age—a fact before demonstrated by the admirable researches of M. Bizot. The orifices are larger than in the adult, and ossifications limited to the aorta entail no irregularity of circulation, nor when uncomplicated, is their presence, he says, revealed by any physical signs. Valvular murmurs are in general not of the same importance as in earlier life, and are often present without any evident disturbance of the heart's functions.

Professor Henderson† has attempted to settle the nature and history of those singular fibro-cysted concretions not unfrequently found in the cardiac cavities. Their organisation, he thinks, is clearly substantiated by microscopic examination; and the observation of Grassmeyer, that clots of fibrine, maintained at a temperature about equal to that of the living body for two or three days, soften into a pulpy matter with the general properties of pus, sufficiently accounts for their varied contents. Polypous cysts, Professor Henderson observes, are contained in those parts where lymph, if exuded, would naturally accumulate. We have only to suppose the latter to become partially organised, and this over the surfaces which are more exposed to contact with the living parts, while in the interior it remains undeveloped or dies, and thus central softening, with apparent purulent formation or

* *Archives Generales de Medecine*, Septembre, 1843.

† *London and Edinburgh Monthly Journal*, September, 1843.

other changes, may be reasonably accounted for. The application of these facts to many statements of Andral, Carswell, and others, is evident.

Three cases of *rupture of the heart*, two of the right and one of the left ventricle, have been reported by Mr. Bodington,* Mr. Challier,† and Mr. Taylor. In neither instance were the valves diseased, and the laceration seemed connected with fatty degeneration.

A very singular case of *carditis* is related by Dr. Leaming, of Philadelphia.‡ A needle penetrated the heart of a girl, aged sixteen, and afterwards entered the chest, exciting sudden and acute pleuritis. From this she recovered. Eight months subsequently she was seized with the symptoms of carditis, and died in three weeks; the needle was found protruding into the left ventricle, passing through the septum and outer side of the right ventricle about three-quarters of an inch from the apex. The pericardium was inflamed, but the lining membrane was healthy.

When attempting to establish a diagnosis between hypertrophy of the right and left sides, Dr. Skoda§ adverts to the necessity of attending to the point of maximum intensity of the *second* sound. If from the aorta, it is best heard near the middle of the sternum, and upwards from that point obliquely towards the right side, for about two inches; while the pulmonary second sound is best heard over the cartilage of the third rib of the left side, or between it and that of the second. A comparison of the relative intensity of the two, conjoined with other signs, would therefore assist in the recognition of the ventricle chiefly affected.

It is well established that considerable disease of the tricuspid valves may exist without producing a bellows murmur. Two cases illustrative of this are given in the *Provincial Medical Journal* for August 12th, 1843. The diagnosis is, in fact, often only inferential, but not the less certain.

Dr. King, of Brighton, relates an instance where the physical signs of hypertrophy of the left ventricle with mitral regurgitation appeared clearly defined, but, after death, proved really to depend

* *Provincial Medical Journal*, November 18, 1843.

† *Lancet*, November 11, 1843.

‡ *Philadelphia Medical Examiner*, from *Medical Gazette*, January 5, 1844.

§ *London and Edinburgh Monthly Journal*, September, 1843.

on pressure, produced by a number of encephaloid tumors. Both the aorta and heart were healthy. Such cases are too often cited, as invalidating the value of physical diagnosis. When viewed correctly, they rather tend to confirm it.

Cyanosis.—Dr. Craigie* has reviewed the facts and opinions hitherto recorded upon the subject of cyanosis, and concludes, that the anatomical cause of the open foramen ovale, hypertrophy of the right ventricle, perforation of the septum, and double ventricular origin of the aorta, is some degree of contraction, or arctation of the pulmonary artery. The open foramen, therefore, instead of being regarded as the cause of death, furnishes the only means by which life can be sustained, while a function so important as that of the circulation through the lungs, is impeded. This mode of reasoning is no doubt applicable to a large proportion of cases; but Dr. Thomas Mayo† relates an instance of a woman, aged 57, who never presented any symptoms of impeded circulation, but whose foramen ovale was found very largely open, the pulmonary artery *dilated*, and both ventricles hypertrophied.

Mr. Harrinson also presented to the Reading Pathological Society a very interesting specimen from a child, aged twenty months, where the foramen was open, the pulmonary artery *enlarged*, the right ventricle hypertrophied, and the left cavities, with the aorta, in a state of atrophy and partial development. The ductus arteriosus was closed, and what was very rare, there was no communication between the left auricle and the pulmonary veins.

A case confirmatory of Dr. Craigie's hypothesis, in a child, aged six months, was read by Dr. Fletcher before the Birmingham Pathological Society, with the addition of transposed aorta and pulmonary artery; and the same observer mentions a very remarkable instance, where the lower half of the body was cyanosed, but not the upper. This depended on the ductus arteriosus remaining open to such an extent as to convey about two-thirds of the blood from the pulmonary artery into the aorta.

It is now well known that in many cases the foramen is open, without any discoloration of the skin. This may be accounted for

* *Edinburgh Medical and Surgical Journal*, October, 1843.

† *Medical Gazette*, February 9, 1844.

by the absence of all obstruction in the orifices or vessels, particularly on the right side of the heart, and a well-balanced condition of the muscular power; it also proves that there are many exceptions to the explanation of this malformation, as offered by Dr. Craigie.

We cannot quit this portion of our subject without recommending a careful study of Mr. Sibson's excellent observations on the diagnosis of diseases of the heart and pericardium.

Digitalis.—Some valuable remarks have been made by Professor Henderson,* as well as Professor Schönlein,† on the indications for the use of digitalis in cardiac affections. In patency of the aortic valves, where the rapidity of the contraction obviates the tendency to regurgitation and its consequences, the effect of digitalis is for the most part injurious; while in diseases of the left auriculo-ventricular opening, both obstructive and regurgitating, it is eminently advantageous. Diminished frequency of beat moderates the force with which the blood is propelled into the lungs, and allows the left ventricle to be more fully distended, if the opening be narrowed; it also lessens the frequency of the interruptions to the passage of the blood, and, in the case of patency of the opening, it prevents the regurgitation from being so often repeated. Much of the reported diuretic power of digitalis may depend on its influence upon the heart's rythm.

To determine the seat of the obstruction, and to estimate the necessity for more or less frequent and vigorous contractions, are the elements which should regulate the employment or rejection of digitalis.

In the treatment of heart affections we are too apt to neglect the state of the general system, and to underrate the practical advantages of a careful discriminating diagnosis.

Pulse.—The frequency of the pulse in newly-born infants, and in children from seven months to six years old, has been investigated with very great industry and research by M. Vallcix.‡

It is, he says, less frequent soon after birth than at the age of six months. The medium frequency when asleep is 87, while the

* *Northern Journal of Medicine*, 1844.

† *Lancet*, May 14, 1844.

‡ *Memoires de la Société d' Observation*, vol. ii., 1844.

normal waking average is from 90 to 100. The slightest elevation of temperature, the least movement or emotion immediately accelerates the circulation. The diminution of the pulse, which Dr. Guy has pointed out, as occurring from the morning to the evening, M. Valleix attributes to the first impression of food upon the system. We should rather ascribe it to the external periodical influences already adverted to. From the seventh to the twenty-seventh month no manifest change as to frequency is observed. The male average of healthy children is 119, and the female 124. From some observations, not sufficiently numerous, the pulse appears to sustain itself a little above 100, up to the age of six years.

The average number of inspirations in children, from seven months to two years and a half old, is between 30 and 32, or one in four, to the number of arterial pulsations.

We may here refer to the observations of the indefatigable Andral,* on the diagnostic uses of the thermometer, both for children and adults.

Whenever, he says, the temperature of an infant exceeds 100° 40′ Fahr., it may be affirmed that fever exists. The highest temperature was observed in typhus, pneumonia, and meningitis. Meningitis may be diagnosed if the thermometer is higher in the first and third than in the second stage, differing in the last from 80° to 95°. Where a high temperature (104° to 105° in the axilla) coincides with a slightly accelerated pulse, typhus may be said to exist, and enteritis if it marks only 100° 40′ to 101° 30′. If the temperature marks 104° to 105°, with a quick pulse and accelerated respiration, pneumonia ensues; on the contrary, bronchitis, if only 100° 40′. In gangrene, paralysis, ague, cholera, and œdema of new-born babes, the temperature is low. If in a child from one to eight years old, the thermometer only marks from 96° to 80°, œdema is to be feared, and its development is certain if the temperature descends from 93° 20′ to 80°. The maximum temperature in adults is 105° 60′; the minimum, 95°. In children, the maximum is 106° 50′; the minimum, 74° 30′.

These investigations are not without interest, but we doubt their great practical utility; nor are we disposed to assume that all M. Andral's results are definitively ascertained.

* *Medical Times*, January, 1844.

Aneurism.—Not less than six cases of dissecting aneurism have been recorded during the past year, by Professor Henderson,* Dr. Lees,† Dr. Todd,‡ Mr. Kirkpatrick,§ and Dr. Peacock.‖ Their individual analysis would lead to no useful result, and they present but little practical interest as regards diagnosis. The last-named observer has very ingeniously and successfully investigated the mechanism of their formation, and his experiments clearly prove that the coats of a healthy vessel cannot be ruptured by any ordinary distending force; that the external coat alone does not possess sufficient power to resist the pressure of the extravasated fluid; and that both theory and observation prove that dissecting aneurism is situated between the laminæ of the middle tunic, and not, as has been generally supposed, between the external and middle coats. Dr. Peacock's experiments also show, that the artificial channel thus formed tends rather to re-open into the original vessel than to burst externally—a result quite in accordance with the description of reported cases.

Professor Henderson¶ relates an instance of sacculated aneurism, which confirms an opinion advanced by him in 1835, that in some cases a bellows murmur follows the diastole of the sac. The existence of such a sound would indicate a narrow communication with the aorta; and were other circumstances favourable for operation, as in aneurism of the inominata, for example, such a mechanical condition would evidently conduce to its success; it would also encourage our hopes when instituting general treatment. On the latter point some valuable remarks have been made by Dr. John O'Bryen, of Bristol,** who insists upon the importance of attempting to induce fibrinous coagulation in the sac, and also of avoiding that excessive depletion and low diet which necessarily render such a result impossible. He advises moderate occasional bleedings, a dry animal diet, the internal use of alum, and the external application of ice or cold poultices, as long as the sensibility of the part will permit. Two cases are reported of a very satisfactory character.

* *London and Edinburgh Monthly Journal*, July, 1843.
† *Dublin Journal of Medical Science*, March, 1844.
‡ *Medical Times*, June 8, 1844.
§ *Dublin Journal of Medical Science*, November, 1843.
‖ *London and Edinburgh Monthly Journal*, October, 1843.
¶ *London and Edinburgh Monthly Journal*, July, 1843.
** *Dublin Journal of Medical Science*, November, 1843.

Dr. Law also considers that low diet should be avoided, and speaks highly of the effects of pills of assafœtida and morphine in relieving the pain and flatulency so distressing in many cases of this disease. He confirms the diagnostic value of the double form of pain, and the fact of the pulsation being single whenever aneurism arose from any other part of the aorta than that upon which the pericardium was reflected.

A very remarkable case of double aneurism of the ascending aorta is recorded by Mr. Judd.* Rupture of the smaller sac into the trachea took place, with alarming hæmorrhage, suffocation, and insensibility, from which he was most unexpectedly recovered by tracheotomy. A second and fatal rupture, fourteen days later, also took place into the trachea. We believe it to be the only instance on record where life was saved by an operation under similar circumstances.

Other examples of aortic aneurism have occurred in the practice of Dr. Stokes, Dr. Law, Mr. O'Ferrall, and Dr. Seymour, but they do not require particular notice.

An interesting case of sudden death, from the bursting of the ascending aorta, in a stout, middle-aged Hindoo, is narrated by Dr. Wise.† Just below the ductus arteriosus the vessel was obliterated, resuming its natural size immediately under, where several large communicating arteries emptied their contents. Congenital contractions are most frequent at those points which undergo considerable change in their size after birth.

Obliterations of the aorta, not congenital, are usually attended with extensive disease of the arterial tunics; but in a case related by Dr. James Duncan,‡ occurring in a woman, aged thirty-two, who died of gangrene of the extremities, the aorta, about an inch above the bifurcation, was closed by a pale yellowish-looking mass adhering to the living membrane, and presenting the appearance of recently effused lymph.

Phlebitis.—Professor Chapman, of Pennsylvania,§ has revived attention to the great value of blisters in arresting and preventing

* *Lancet*, April 6, 1844.

† *Provincial Medical Journal*, July 22, 1843; from *Bengal Times*.

‡ *London and Edinburgh Monthly Journal*, September, 1843.

§ *American Journal of the Medical Sciences*, July, 1843.

traumatic phlebitis. He advises a strip of blister plaster to be applied over the whole length of the inflamed vein, cutting an opening at the orifice, which is to be covered with a soft poultice:—"With entire rest of the limb," he says, "I have reason to suppose that blistering will arrest a very large proportion of these cases, antecedently to the suppurative stage, and even then when the attack is mild."

DISEASES OF THE ABDOMEN.

Stomach.—Sir Henry Marsh* has described a particular condition of the stomach, characterised by regurgitation of its contents, and which has not hitherto obtained separate consideration. It assumes the form of a very distressing and intractable disease, is frequently associated with hysteria, pregnancy, and tubercular affections, and is peculiarly met with in persons of a strumous diathesis. All causes which lower the vital energies seem capable of inducing it; and Dr. Marsh has found no remedy so efficient as total change of air and habits, enjoining the recumbent posture after meals, with a dry and spare diet. Such cases appear to include those which have been recorded under the title of "human rumination."

Perforation of the Stomach.—Several treatises upon this important lesion have been written by continental physicians during the last fifteen years, and numerous cases and short essays upon the subject are scattered through British periodicals. A valuable digest of five instances, reported in the latter, has been made by Mr. Cross, of Walworth,† who concludes, that perforation is much more common in women than in men (39 to 12); that it is situated in the former in the *left half of the stomach,* in the smaller curvature near the cardiac orifice; while in the male it is generally *near the pylorus;* that perforation rarely, if ever, occurs in the female before the age of *puberty,* and seldom after the cessation of the menses; that the disease is very rare in the married female; and that a chlorotic condition of the system is the chief predisposing cause. Of 39 females, 36 were between the ages of

* *Dublin Journal of Medical Science,* July, 1843.

† *Lancet,* August 5, 1842.

fifteen and thirty, while only one male was under twenty. Dr. Pritchard, of Leamington, in his observations on hysteria, had before arrived at similar conclusions.

The diagnostic symptoms are, *violent* and *sudden* pain in the region of the stomach, extreme anxiety of countenance, and irregular spasmodic contractions of the abdominal muscles.

Dr. Seymour* thinks there is no foundation for supposing any form of ulceration of the stomach as peculiar to females; but Mr. Crisp† has, we think, in a subsequent communication, established the reality of his first conclusions. It is difficult to imagine any plausible explanation of so singular a fact. Bleeding, with the free use of opium, appears to have prolonged life.

Mr. Goodard‡ relates a rather exceptional case in a male, aged twenty-three, where the ulcer was situated in the smallest curvature, and near the cardiac orifice.

Perforations of the *duodenum* are very rare. A remarkable instance is mentioned by Dr. Ash, of Philadelphia,§ occurring in a healthy, robust man, aged fifty, and whose organs presented no other evidence of disease. He survived forty-four hours.

A very similar case, where the ileum was the seat of the lesion, is detailed in the *Edinburgh Monthly Journal*, for December, 1843. It proved fatal in twenty hours and a-half; it had evidently commenced in the serous membrane. No other morbid affection was detected after death.

Against the obstinate vomiting connected with pregnancy, Dr. Pitschaft strongly recommends pills of creasote and henbane powder three times a day.

Obstipatio.—Mr. J. Sproule speaks very highly of the efficiency of a decoction of colchicum seeds in all cases of constipation. His formula is two drachms of the seeds to two pints of water, which is to be boiled half an hour and then strained. The dose varies from one to two tea-spoonfuls three or four times a day. This mode of preparing colchicum is, he thinks, most to be relied on in rheumatism.

* *Royal Medico-Chirurgical Society*, November 14, 1843.

† *Lancet*, December 22, 1844.

‡ *Lancet*, August 29, 1843.

§ *Dublin Journal of Medical Science*, January, 1844.

Dr. Favell* mentions a case of ileus in a lady, which yielded, after three quarters of an hour, to the action of galvanism.

Colica Pictonum.—In a clinical lecture on this disease,† M. Piorri concludes, that the pathological condition is paralysis of portions of the digestive tube, similar to what is witnessed in the voluntary muscles. Pain is the consequence either of the contraction of the non-paralysed parts, or of gaseous distension. In about thirty cases he found the dulness on percussion over the cæcum or colon ascendens, while the small intestines were clear; in others, the dulness corresponded to the sygmoid flexure. He has invariably found the rectum flaccid, and without contractility.

The indications of treatment are to favour the excretion of the poison by the different emunctories, and to accomplish this he enjoins abstinence from solid food, and prescribes copious diluents by mouth and rectum, tepid baths, and mild purgatives, if necessary. Under this plan the patients recovered in five or six days. He doubts the utility of sulphuric acid, stating that in the lead manufactory of Clichy, its use had been relinquished. M. Piorri, in his laudable desire to establish a rational treatment, is hurried on to undervalue the success of means which are empirical. To analyse the organo-pathological conditions first, and then to select the appropriate remedy, is an admirable rule, but not always either possible or practically the best. A great deal of empiricism will ever attach itself to practical medicine. The art will ever be in advance of the science, though the latter is, unquestionably, the most to be relied on for future progress.

Peritonitis.—Dr. Willis‡ regards the serous membranes as contrivances for the accommodation of large numbers of lymphatic vessels, and thus accounts for the serious effects of their inflammation on the general system. His views on the different capacities of endosmose and exosmose, continually engendered between the blood-vessels and the liquor sanguinis, and between the liquor sanguinis and the lymphatics, are very ingenious and interesting.

The diagnosis and treatment of tubercular peritonitis has met

* *Provincial Medical and Surgical Journal*, June 12, 1844.

† *Medical Times*, April 20, 1844.

‡ *Medical Gazette*, April 19, 1844.

with careful investigation by Dr. Clymer, of Philadelphia;* but his views are only confirmatory of the well-known conclusions of Louis.

A very singular and unaccountable case of constipation and peritonitis, simulating all the leading general and local indications of advanced pregnancy, is related by M. Barbieri, of Fifeshire.† It is minutely detailed, and accompanied with some very sensible observations.

Cholera.—Mr. Rankin, in a letter to the Medical Board of Bengal,‡ states that he considers cholera to depend on an obstruction of the passage of the chyle and its regurgitation into the intestines. The practice proposed is, fortunately, better than the theory, viz.—a hot bath, from 110° to 115°, and continued for an hour or more, until reaction is established. Internal medicines he rejects. Mr. Clark, another Indian medical practitioner,§ says, "our sheet anchor was the vapour bath, with venesection in the bath when reaction took place."

A Mr. Preston, of Cuddalore, adopts another speculation, viz.—that cholera is attributable to some telluric electric change, probably arising from volcanic agency, reducing our nervous energy, and enfeebling the heart's action. From its supposed influence as a stimulant to the sympathetic nerves, he is anxious that Secale should be tried.

A German physician‖ recommends the acetate of lead with strychnine, and a second the acetate of lead with tannin. Of the above, the hot bath alone deserves any confidence.

Liver.—An interesting example of biliary obstruction, from pancreatic disease, is narrated by Dr. Williams,¶ where scarcely a nucleated cell was discovered, and nothing but *free* fatty particles and granulated matter could be seen. This condition must have impeded secretion, and thus prevented biliary accumulation. In all similar cases it would be interesting to ascertain the conse-

* *Philadelphia Medical Examiner*, November 11, 1843.
† *London and Edinburgh Monthly Journal*, March, 1844.
‡ *Medical Gazette*, November 4, 1843.
§ *Medical Gazette*, April 5, 1843.
‖ *Medicinische Zeitung*, July, 1843.
¶ *Guy's Hospital Reports*, October, 1843.

quent changes in the deposit of fat, and in the amount of urea excreted.

Dr. Williams observes, that in fever, conditions exactly the reverse of those obtaining in pulmonary disorganization prevail. The microscope proves that the cells are then destitute of fat. In a granular state of the liver the colouring matter is gone, and the cells are semi-transparent. Disease of the cells may be said to mutilate the tenderest elements in the machinery of the gland. Becquerel remarks, that in advanced stages of cirrhosis the urine is scanty, orange-yellow, very deep coloured, and charged with a large quantity of uric acid.

Dr. Graves* records a case of jaundice, fatal after seven weeks, and where the only morbid appearance was inflammation of the gall-bladder. The patient had yellow vision, and the pruritus preceded the discoloration of the skin.

A remarkable instance of rupture of the gall ducts is recorded by Mr. W. R. Barlow,† of Essex. It was followed, after sixteen days, by a swelling, from which, at six different tappings, between the 9th of October and 27th of November, 1843, not less than *thirty quarts* of fluid, having all the appearance of bile, were drawn off. The patient was quite well in the beginning of February. This immense quantity of bilious fluid is a strong fact in favour of the bile not being merely excretory. The effect on the nutritive process, according to Liebig's views, ought to have been very great under these circumstances.

Suppurative inflammation of the liver was the subject selected by Dr. Budd‡ for his interesting "Gulstonian Lectures." He ascribes it to phlebitis, more or less remote, and points out the singular connection between hepatic abscess and ulcerations in the large intestine. Why abscesses in some cases form in the lungs, in others in the liver, he explains by the distribution of the affected veins, and draws an ingenious contrast between the seat and multiplication of disseminated cancerous masses and purulent formations. He furnishes additional proofs that the supposed frequency of duodenal ulceration and hepatic disease is erroneous. Contamination of the blood with pus leads to circumscribed

* *Dublin Journal of Medical Science*, November, 1843.

† *Royal Medico-Chirurgical Society*, May 14, 1844.

‡ *Medical Gazette*, October 13, 1843.

deposits; but if with a volatile or readily diffusible poison, to diffuse inflammation. Dr. Budd's observations possess considerable practical interest.

Spleen.—The occurrence of hæmorrhagic affections, and the frequency of ulcerations of the lower extremities in patients affected with chronic enlargements of the spleen, have attracted the attention of several pathologists. An illustrative case of both these attending conditions has been reported in the *Provincial Medical Journal*, of July the 8th, 1843. The patient had been exposed to malaria, but had never suffered from intermittent fever. Towards the close of life, however, periodical febrile paroxysms were distinctly present. It will be recollected that M. Piorri maintains that the true pathology of ague is enlargement of the spleen, and that the latter is most successfully combated by the free employment of quinine.

Pancreas.—The diseases of this organ are confessedly ambiguous and obscure. In the *Dublin Medical Journal* for May, 1844, will be found a very intelligent communication on the subject, by Dr. Battersby. He particularly directs attention to the sympathy existing between the pancreas and the salivary glands, as a point well worthy of attention when attempting the diagnosis of pancreatic disease. The fact of their disturbance co-existing with obscure gastric derangement would militate strongly in favour of the pancreas being affected. This organ he also considers to be the principal seat of pyrosis.

Diabetes.—Dr. John Percy* has continued his elaborate investigations and experiments on this most singular and, as yet, but partially understood disease. He confirms the important fact of sugar co-existing in the blood, stomach, intestines, and some other secretions, as before noticed by M'Gregor, Rees, Maitland, Bouchardat, and others. Grape or cane sugar, he has proved in dogs, may circulate with impunity to the nervous system, and is excreted by the kidneys unchanged. He also confirms Dr. M'Gregor's assertion, that grape sugar may be formed out of previously assimilated matter, or out of the protein series of

* *Medical Gazette*, July 28, 1843.

compounds of the food. Dr. Aldridge* adopts a similar view, and assumes that albumen may be its occasional source, the loss of water and oxygen converting it into sugar and ammonia. In favour of this he states, that diabetic urine is invariably *neutral* when first passed, (an assertion denied by Dr. Percy,) and he has also found the amount of albumen proportionately less in diabetic than in healthy blood. Additional researches are wanted to test the accuracy of Dr. M'Gregor's statement, that sugar is found in the stomach of patients restricted to an azotized diet; also, whether there is any constant relation between the proportion of sugar in the urine and the quantity of fecula or saccharine matter in the ingesta.

M. Mialhe† ascribes diabetes to an excess of acid in the blood, the consequence of suppressed cutaneous functions. A free alkali he considers essential for the assimilation of sugar, and his treatment consists, of course, of antacids and sudorifics.

Dr. Percy‡ remarks, that during the restriction of the patient to an animal diet, the amount of the urea is increased, so that, in accordance with the recent researches of Lehmann, a direct relation appears to exist between the nature of the diet and the proportion of urea in the urine. In extraordinary circumstances it is, says Dr. Percy, easy to understand by reference to formulæ, how a protein compound can supply carbon for pulmonary oxidation, and yield grape sugar and urea.

Dr. T. Thompson§ reports a case as illustrative of the injurious influence of *bread* as an article of diet. We have ourselves detailed an instance of the same class, where we found a small quantity of bread twice reproduce the local and general symptoms; and we cannot doubt that its use should be entirely prohibited when attempting the cure or alleviation of diabetes.

Dr. Favell‖ states that he has derived considerable advantage from eight to fifteen drops of the hydrosulphuret of ammonia three times a day; and Dr. Chossat, of Geneva, advises the free use of fatty substances, to replace the excessive loss of hydrogen and carbon which the system experiences by the production of sugar.

* *Dublin Journal of Medical Science*, January, 1844.
† *Medical Times*, April 27, 1844.
‡ *Medical Gazette*, May 5, 1844.
§ *Provincial Medical Journal*, August 5, 1843.
‖ *Provincial Medical Journal*, September 16, 1843.

To Dr. Golding Bird* we are indebted for an able comparative examination of the different plans which have been proposed for testing diabetic urine. He gives a very decided preference to Trommer's method, or the copper test, over every other, both for accuracy and convenience; the growth of *Torula*, the development of fermentation, and Runge's sulphuric test, have the next claims upon our confidence in the order of their citation.

Mr. George Ross, in the *Lancet* for June 14, 1844, suggests the following process:—Add some common cheese to the urine, and after letting it stand for some hours, add some acetate of zinc or magnesia: if sugar be present, a white precipitate will be thrown down. Liebig has before proved the formation of lactic acid from sugar, by the contact of casein.

It is quite evident from the above, that the pathology and treatment of diabetes are awaiting further elucidation.

Albuminuria.—We as yet know but little of the etiology or treatment of this disease. We are acquainted with the fact of albumen existing in the urine, and are tolerably sure of finding after death a certain change in the texture of the kidneys, but we are unable to define where the disorder originates, and are unacquainted with any direct means of combating the condition. The past year has not been destitute in conjectures upon the subject.

Dr. Malmsten, of Stockholm, has carefully analysed 124 cases occurring in his own practice, and concludes that albuminuria depends essentially upon a morbid condition of the blood, exciting functional, and afterwards organic changes in the kidneys.

Another writer,† who indulges in laborious historical research from Hippocrates downwards, adopts a similar hypothesis, admitting an augmenting, if not an originating power in the kidney itself; and Dr. Heaton,‡ in his valuable articles upon different forms of granular disease of the kidneys, concludes that it depends on a deposit of lymph, of an imperfect character, affecting first the cortical, and afterwards the tubular structure. This may depend on constitutional causes only, causing a deviation in ordinary

* *Medical Gazette*, November 24, 1843.

† *Journal des Connaissances Medico-Chirurgicales*, Mars, 1844.

‡ *Medical Gazette*, March, 1844.

textural nutrition, or it may be aided or excited by renal inflammation, when the lymph may be poured out as an inflammatory effusion, as well as in connection with the nutritive process.

The recent experiments of M. Fourcault clearly prove that the urine becomes albuminous when the functions of the skin are artificially arrested—a view of the disease advocated by Mr. George Ross, at least six years ago.

Dr. Aldridge* has detected albumen in the urine in many cases of gonorrhœa, and considers its presence a good practical indication for abstaining from stimulating treatment. Pus, on the contrary, when secreted from the urinary passages, requires, he says, tonic and stimulating measures.

The conditions, both local and general, associated with albuminous urine, are evidently too varied and complicated to admit of any common expression. Until these are very clearly defined, and our knowledge of the nutritive process materially advanced, but little practical good will result from the most ingenious speculations. The fact is, however, not without present importance, or incapable of useful application to practice.

Urine.—Dr. Aldridge,† in his "Notes on Urinary Diseases," questions the soundness of Liebig's opinion, that the modifications of the urine are produced by variations in the oxydation of the waste of tissues, and infers that in disease the urine is more frequently characterised by alterations in the quantity of water, than by changes in the properties of its organic elements. The diagnostic value of the urine he thinks very great. Although strabismus, convulsions, delirium, heat of head, and bounding carotids should all display themselves, if it remained pale and transparent, we might be certain there was no inflammation.

The existence of iron in the urine has been affirmed and denied by many. In eleven cases under the use of steel, Dr. Aldridge was unable to detect the slightest trace. This metal seems rather essential to the blood globule, than to form a constituent part of our tissues.

According to Professor Liebig,‡ the acid, neutral or alkaline

* *Dublin Journal of Medical Science*, January, 1844.

† *Dublin Journal of Medical Science*, January, 1844.

‡ *Lancet*, June, 1844.

reaction of urine in healthy individuals, does not depend upon any differences in the processes of digestion, respiration, or secretion, but upon the alkaline bases of the food. Hence, he says, (for the consolation of all former observers,) it follows that the analysis of urine, when made without respect to the inorganic salts, acids, and bases of the aliment, teaches nothing whatever, and justifies no healthy nor morbid inference. He believes there is now required only a small number of correct observations, to establish a fixed rule for the remedies necessary in various cases. Future experiments will prove whether sanguification is absolutely dependent upon the presence of alkaline phosphates or not, salts to which he ascribes very important properties. We shall also be able to determine whether weak solutions of alkaline phosphates are not the best solvents for uric acid in the bladder, and what is the influence which aliments, rich in sulphur, such as mustard, for instance, exercise upon the separation of uric acid in the bladder, by the formation of sulphuric acid. At any rate we may expect, by a judiciously selected diet, to alter, with certainty, the nature of the urine. We may keep it alkaline for a long time, without injury to health, by adopting a vegetable diet, and this is certainly the first condition necessary to prevent the formation of uric acid. By its combination with an alkaline base, uric acid must, in the organism, resolve itself into its ultimate oxygen compounds with the same facility as other organic acids, if the physician prohibits all substances which, like wine or fat, take possession of the oxygen necessary for transforming uric acid into carbonic acid and urea.

Chemistry has now the ascendant, and the illustrious Liebig occupies the papal chair; but we doubt not, as Dr. Todd has already ably proved, that the bedside practitioner will meet with many facts obstinately heretical to laboratorial creeds, and discover that vital acts refuse to be resolved by even the magic crucible of Giessen. The reader is referred to some very able critiques upon the value of Liebig's doctrines, published in the *Medical Times* for the present year; and in Dr. Caldwell, of America, this illustrious chemist has found a most decided, but not a very temperate, nor even a very reasonable assailant.

Dr. G. E. Day,* by more than 200 fresh analyses, has demonstrated the great incorrectness of Becquerel's table, so universally

* *Lancet*, June 15, 1844.

adopted, which professes to give the amount of solid residue in 1000 parts of urine, from the simple knowledge of the specific gravity. If, very justly observes Dr. Day, all the various constituents of the urine possessed the same specific gravity, or even if the different constituents of the solid residue were always associated in the same relative proportions to each other, and if no abnormal constituents, as albumen, sugar, pus, &c., ever presented themselves, we should experience no difficulty in the establishment of a formula, which would express a constant relationship between the specific gravity and the amount of solid residue in a known quantity of urine. As however these conditions do not occur, it is clearly impossible to construct a formula which will give mathematically accurate results. If in a state of health, such an attempt is hopeless, much more so in a state of disease.

Dr. Day has also tested the well-known diabetic table of Dr. Henry, by 25 additional analyses, and finds that the mean error by Becquerel's formula amounts to 21.92, and by Dr. Henry's, to 7.71, while by adopting the modification proposed by Dr. Christison, viz., of substituting 2.33, instead of 2.58, as the multiplier, the approximation is sufficiently near for all practical purposes. Dr. Day deserves much praise for his laborious and useful investigations.

A case of *ischuria renalis* is related by Dr. Theophilus Thompson,* where no urine was secreted for 120 hours. During the succeeding forty hours, not more than half an ounce was passed, and the natural quantity was not restored for more than a month. No head symptoms occurred. In addition to bleeding, cupping, and the warm bath, Dr. Thompson places much reliance upon issues, and thinks well of the nitrate of silver applied for this purpose to the loins.

Against *incontinence of urine*, Dr. Brown,† of America, recommends ten grains of nitrate of potass, every three or four hours. He supposes it to act by rendering the urine more stimulating.

Mr. Leney, of Bray,‡ relates a severe case of *renal hæmorrhage* resisting all ordinary measures, but which rapidly yielded to injecting the colon with cold water.

* *Medical Gazette*, January 12, 1844.

† *Provincial Medical Journal*, July 8, 1843.

‡ *Provincial Medical Journal*, February 10, 1844.

Benzoin water, charged with two volumes and a half of carbonic acid, and containing, in sixteen ounces, fifteen grains of purified benzoate of potass and bi-carbonate of potass, promises to prove a pleasant and convenient formula in many cases of digestive and urinary derangement.

A curious case of *congenital malformation* of the kidneys is recorded by Dr. Sandwith, of Hull.* Towards the close of life a forcibly pulsating tumour was discovered in the umbilical region, with a distinct bellows murmur. It was formed by an intervening portion of the kidney overlaying the aorta and uniting the renal apparatus into one semi-lunar organ. The physical conditions were rendered more complicated by a considerable exostosis of the third lumbar vertebra. No diagnostic tact was equal to such a strange conjunction of difficulties.

Dr. Ure† has pointed out the remarkable affinity of the carbonate of lithia for uric acid, and suggests its employment as a solvent for stone. At present, however, the article is too scarce for extensive experiment.

Ovarian Dropsy.—It would almost require a small volume, instead of a paragraph in a Retrospective Address, to give a digest of all that has been said and done on the subject of ovarian disease. The great question revived and agitated during the past year has been the propriety of removing ovarian tumours by surgical interference; and though the opinions of professional men are still far from convergent, yet ample evidence has been afforded that the danger of extraction is less than has been apprehended, and that there *are* cases where ovariotomy may be most judiciously recommended and successfully performed. How to distinguish such cases; how to avoid mistakes in their diagnosis; what are the attending circumstances adverse or favourable to operative interference; whether the incision should be long or short; whether tapping should be previously performed, or the tumour extracted entire; what amount of adhesion should render the operation unsafe, and what are the probabilities of success when contrasted with major operations; whether, in short, extraction of diseased ovaries is ever to be practised, or to be relinquished

* *Provincial Medical Journal*, February 10, 1844.

† *Pharmaceutical Journal*, August, 1843.

as a rash and dangerous proceeding—are considerations which have been most zealously discussed and most oppositely decided. From the admirably detailed cases of Dr. Clay, Mr. Walne, Dr. F. Bird, and Mr. Southam, besides other instances reported from the continent and America, not to mention the statements made by earlier observers, (for the operation dates back at least seventy years,) we believe the value and rationality of extraction, under favourable circumstances, to be satisfactorily demonstrated; and in the statistics of the operation so diligently collected by Mr. Phillips, Dr. Churchill, and a writer in the *British and Foreign Medical Review*, we can discover nothing which would justify a more unfavourable conclusion, because materials of a most dissimilar and heterogeneous character form the elements of their calculations, and are utterly incapable of eliciting truth. The numerical system so applied becomes a positive abuse, and gives a sanction to results which have nothing to recommend them but the garb of arithmetical expression. If the attendant risk, when properly performed, be not greater than that of major operations generally, and if the inefficiency of internal treatment be ascertained, we regard all discussions on diagnosis and method of performance, as of very secondary interest, because every case must, after all, be individually considered, and the same general principles must be acted upon as would influence our decision under other, but analogous circumstances.

Narrowing limits compel us to restrict our observations to general conclusions, but we shall append the following references we had made for a more detailed exposition of the subject, thinking they may prove convenient to those who are desirous of knowing what has been said and accomplished during the past year:—*Medical Gazette*, August 11th, 18th; October 13th; November 16th; December 8th, 29th, 1843; February 23rd, 1844—*Lancet*, December 15th, 1843; May, 1844—*Provincial Medical Journal*, November 11th, 18th, 1843—*Medical Times*, July 17th, October 7th, 1843; March 16th, July 6th, 1844—*British and Foreign Medical Review*, October, 1843—*Edinburgh Medical and Surgical Journal*, October, 1843—*London and Edinburgh Monthly Journal*, January, 1844—*Dublin Journal of Medical Science*, November, 1843; July, 1844—*Guy's Hospital Reports*, October, 1843—*American Journal of the Medical Sciences*, January, 1844.

We felt it to be cheering in the midst of so much surgical strife, to meet with a writer who can boast his triumphs independently of the knife. Mr. Brown* reports four cases as having been cured by the following treatment:—He advises mercurial frictions on the abdomen until the mouth is slightly affected—a state to be sustained for some weeks. Diuretics are to be administered, and tonics after the first week; the diet to be light and nourishing, and the patient to take daily exercise. The local treatment consists of *very tight* flannel bandaging; and if the cyst remain stationary, or evince a tendency to increase, tapping must be employed, followed immediately by accurate padding, with folded napkins and a *tight* bandage, for two or three weeks. The mercurial friction is also to be continued at least six weeks after the operation. These means are not new, but their methodical employment, as advised by Mr. Brown, is certainly worthy of attention.

Dr. Ollenroth,† in an interesting monograph on the curability of this disease, again advocates Richter's operation, which consists in changing the palliative intention of paracentesis into a radical cure, by retaining an elastic tube in the sac, preventing redistension, and favouring adhesive inflammation. By numerous practitioners this procedure is regarded as more hazardous than even excision.

Fibrous Tumours of the Uterus.—Dr. Cambernon‡ offers a very plausible and probable explanation of the cause and frequency of fibrous tumours and polypi of the uterus. He regards them as originating in unimpregnated ovules, thrown off under varying circumstances of uterine excitement, and finding their way by vascular or other channels into the tissues of the uterus, where they undergo a peculiar growth and evolution. From extensive statistics he finds that they never appear prior to puberty; that they are chiefly found in females who have menstruated regularly, borne children, and evinced the general signs of sexual activity, while the traces of bone or hair with which they are sometimes associated, would greatly tend to confirm the fact of their originating in ovarian cells.

* *Lancet*, May 4, 1844.

† *Medinische Zeitung*, March, 1843.

‡ *Gazette Médicale de Paris*, Fevrier 3, 1844.

Uterine Hæmorrhage.—The efficiency of opium in large doses in arresting uterine hæmorrhage was, we believe, first insisted upon by Dr. Stewart Duncan, in 1816; and though subsequently recommended by Professors Burns and Davis, has not been practically recognised by the profession.

Mr. Thomas Dorrington, of Manchester,* has adduced some very valuable facts and reasonings in favour of this mode of treatment, if energetically and fearlessly enforced. He advises a drachm of the tincture to be repeated once, twice, or oftener, at intervals of five, ten, or fifteen minutes, until the general soothing effects have been obtained, stating that the after effects are all favourable to the safety and advantages of the plan. We cannot over estimate the value of any treatment adapted to such trying and difficult emergencies, and strongly recommend a perusal of Mr. Dorrington's paper.

In all cases of sudden sinking after injuries or operations, a similar mode of administering opium might be reasonably tried. It is an excellent remedy, and its uses even yet but partially known.

Dr. Stevenson† strongly recommends the use of gallic acid, in doses of six to eight grains every three or four hours. Mr. Wardleworth‡ asserts that he has never failed with two or three grains of the ergot, in the same intervals; while on the other hand, Dr. Toogood, of Bridgewater, in his excellent remarks on the practice of midwifery,§ says—"It has not been successful in my hands." MM. Aran and Sauter speak very highly of the effects of the powder of the *Juniperus Sabina*,‖ in doses of from fifteen to twenty grains, in hæmorrhage, and as a valuable stimulant in all cases of atony of the generative organs.

Uterine Sound.—In the *London and Edinburgh Monthly Journal* for August and September, 1843, an elaborate description will be found, by Professor Simpson, of his uterine sound, intended to explore the internal surface, and to ascertain the positive size and mobility of the uterus.

* *Provincial Medical and Surgical Journal*, April 10 and 17, 1844.
† *Provincial Medical Journal*, July, 1843.
‡ *Provincial Medical and Surgical Journal*, May 8, 1844.
§ *Provincial Medical and Surgical Journal*, May 22, 1844.
‖ *Provincial Medical and Surgical Journal*, June, 1844.

Pruritus Pudendi.—Lotions of borax have been successfully employed, and its internal administration advised.

Spermatorrhœa.—The treatment of the wretched consequences of sexual excesses, exclusive of syphilitic and blenorrhagic affections, has too generally, observes Dr. Ranking,* been consigned to a section of that horde of unprincipled pretenders, which the Government of this country continues to tolerate and indirectly to patronise, and from whose obscene advertisements it condescends to derive a portion of its emoluments. It is, indeed, to be desired that this most wretched of all the curses which man's imprudence entails upon him, were thought more worthy of the time and attention of the educated practitioner. Dr. Ranking adopts the pathology and treatment of Professor Lallemand, and considers the application of lunar caustic to the affected portion of the urethra, the most direct and efficient means. The question in its moral bearings is also carefully considered, and some useful suggestions given as to the general management of the patient.

Dr. Dangerfield,† in a subsequent communication, advocates very similar views; and Mr. Douglas, of Glasgow, recommends the employment of sedative injections, as equally efficient with caustic applications.

A good deal of discussion has taken place on the causes of this painful condition, but as it has involved moral rather than medical considerations, we shall content ourselves with simple reference to the fact. It is a little singular that no allusion is made to the state of the cerebellum by either of the belligerent parties.

DISEASES OF THE SKIN.

It appears, from a memoir by Dr. Danielsson,‡ of Bergen, that two forms of elephantiasis, the one affecting the skin, the other the glands, are endemial in parts of the Norwegian coasts. Out of 200,000 inhabitants 1,200 are at present affected. A deposit of a hard, granular, and yellowish substance is found in the dermis, cellular membrane, parietes of the veins, larynx, trachea, bronchi,

* *Lancet*, October, 1843.

† *Lancet*, November 17, 1843.

‡ *Medical Times*, April, 1844.

pleura, liver, spleen, uterus, and intestines, but, what is remarkable, not in the lungs. The true nature of the disease it is difficult to surmise; there is no evidence of its contagion; and it may be induced, in a healthy person, by all circumstances which are unfavourable to the general health. It resembles a modified form of scrofula more than any other condition.

In New Brunswick* a chronic and malignant form of cutaneous disease has been making slow but steady progress for the last twenty-seven years. A special commission reports its identity with the tubercular leprosy of the middle ages, and with what has recently been prevalent in the Feroe Isles, Shetland, Holstein, Madeira, the Crimea, Africa, Ceylon, and the East and West Indies. It is considered contagious, and strict seclusion recommended.

Dr. Troupes,† of Turin, has also reported the existence of one hundred lepers in the Sardinian States, only three of whom presented all the characters of the ancient lepra.

The button scurvy in Ireland has been recently described and christened "Ecthyma globulus" by Dr. Patterson.‡ It appears, in its present form, to be a very curable and unimportant disease.

A very singular epidemic, affecting 80 individuals out of a population of 700, has been described by M. Aliès.§ It consisted of an eruption of mucous tubercles in the neighbourhood of the anus and genitals, closely resembling syphilis, and rapidly yielding to the use of mercury.

The more intimate knowledge now possessed of the syphiloides, seems to suggest an obscure relationship between the Radesyge and Spetalska of Norway, the Sibbens of Scotland, the Ocarliero of the Adriatic, the Morbus Crimeus of the Black Sea, the Aleppo Pustule, and the Yaws of the West Indies, &c. It is also possible that the different forms of leprosy may belong to this group, but peculiarly and locally modified.

Dr. Babington || speaks favourably of a lotion with the impure pyroligneous acid, suitably diluted, in cases of ordinary lepra.

* *Medical Gazette*, June 14, 1844.
† *Annales de Therapeutique*, 1843.
‡ *Dublin Medical Press*, March, 1844.
§ *Journal de Medecine de Lyons.*
|| *Medical Gazette*, September 8, 1843.

In the treatment of *tinea capitis*, no plan, says Mr. Wigan,* is to be compared with the use of a lotion composed of one part of Beaufoy's concentrated acetic acid and three of water. The first application is often sufficient.

M. Dauverger recommends the sulphate of iron, both in solution and powder, in cases of *sycosis* and *acne*.

The microscopic parasite of the sebaceous sacs and hair follicles, discovered by Dr. Simon, of Berlin, Mr. Owen proves not to belong to the class of entozoa, but to represent the lowest organised form of the class arachnida, making a transition from the arachnida to the higher articulata.

A very curious case of what Dr. Simpson calls "icthyosis intra uterina" has been described by Dr. Keiller,† of Dundee. The entire dermoid covering was hard, preternaturally thickened, and deeply fissured. Five analogous cases are referred to, but the condition is evidently one of very rare occurrence.

Professor Simpson has carefully investigated some cases of the "skin bound disease of infants," and has found the urine coaguable and the kidneys striated, from which facts he concludes that it is merely a form of anasarca, with the occurrence of Bright's disease in the earliest period of infancy.

ENTOZOA.

Filaria Medinensis.—Very few opportunities occur of observing this singular species of entozoa in our own climate, but a well-marked case has been very recently described by Dr. Oke,‡ where three of the filariæ existed in the same individual, two of which were safely extracted by gradual and cautious traction. The general and local symptoms minutely corresponded with the statements of distant observers.

PRACTICAL MEDICINE AND MATERIA MEDICA.

Counter Irritation—Blisters.—The application of counter-irritants, and in particular blisters, is too often regulated by empirical routine rather than enlightened discrimination. Much useless suffering is

* *Medical Gazette*, September 8, 1843.

† *London and Edinburgh Monthly Journal*, August, 1843.

‡ *Provincial Medical Journal*, August 26, 1843.

not unfrequently inflicted, and injury, either from excess or deficiency, is too often the result. Mr. Atkinson* has made some very useful observations on the subject, and pointed out a principle by which we may be guided in the mode of employing counter-irritant remedies. He considers an estimate of the patient's temperament, and of the tone and frequency of the pulse, to be a much surer guide than that of age. The greater the irritability, the greater the sensibility of the cutaneous surface. If the pulse be turbulent, wiry, and from 100 to 170, from ten minutes to half an hour Mr. Atkinson considers sufficient time for a blister to act. Should the pulse be small and wiry, and the frequency from 80 to 100, from a quarter of an hour to two hours may suffice, but a longer time is necessary in all cases if the skin be moist. When the skin is soft and the pulse under 70, a blister may be safely applied from twenty to forty hours. These general rules, intelligently modified to ever-varying circumstances, Mr. Atkinson has found, after many years of careful observation, of great practical assistance and utility.

The dangerous and often fatal effects of imprudent blistering in children has been freely discussed during the past year.† Various opinions have been expresssd, but beyond the recommendation of limiting the time in children to a period varying from half an hour to three hours, no general principle has been established.

To obtain an immediate blister, M. Daucq‡ recommends the following plan:—Drop into a flattish watch glass eight or ten drops of the strongest liquid ammonia; cover it with a piece of linen of rather less diameter than the glass; apply the glass so prepared to the skin, and keep it in place by moderate pressure with the finger. As soon as a red zone can be perceived we may be sure that vesication is accomplished, when the surface may be washed and the epidermis removed. In most cases the process is completed in thirty seconds.

The following liniment is particularly recommended by Dr. Morris,§ when desirous of keeping up a mild rash upon the skin: ℞. Ol. Crot. M. xx.; Antim. Tart., ℈j.; Liq. Potassæ, ʒj.; Aq. puræ,

* *Lancet*, May, 18, 1844.

† *Lancet*, August 5 and 11, 1843.

‡ *Provincial Medical Journal*, February 3, 1844.

§ *Provincial Medical Journal*, September, 1843.

ʒvij. M., while Dr. Hannay,* of Glasgow, considers ipecacuanha one of the most manageable and efficient applications. His formula is Pulv. Ipecac. Ol. Olivæ, ää ʒij.; Adipis. Suill. ʒss. M. It requires to be rubbed in for fifteen to twenty minutes, and is, he thinks, peculiarly adapted to cases of cerebral irritation in children, dependent on receded eruption.

Anodynes.—We have but little that is either novel or interesting to report on the subject of materia medica. The use and abuse of opium in children have been carefully considered by Professor Beck, of New York,† who, perhaps, errs by too great an apprehension of the danger of this drug in the earlier periods of life. Laudanum, paregoric, and Dover's powder are the preparations he prefers, advising the use of smaller doses than those usually recommended, and that they should not be frequently repeated.

Professor Beck strongly insists upon the necessity for the strictest prohibitions against the administration of opium by non-professional persons, and adverts to the fearful evils resulting from its ignorant administration in many quack medicines, of which abuse the current year has afforded several distressing examples. When habitually given, the effect is to stunt the growth of the child: it is emaciated and puny; the skin is flabby and shrivelled; there is an absence of all intelligence, and the whole appearance is haggard, presenting a sort of "miniature of old age."

Dr. Smith, of New York,‡ directs attention to the influence of opium upon the reproductive organs of women, and relates five cases where the catamenial secretion was permanently arrested or suspended for considerable periods in patients habituated to its immoderate use. Dr. Smith remarks, that in no instance was amenorrhœa, under these circumstances, productive of any constitutional disturbance.

The *Cannabis Indica*, or Indian hemp, long known in the East, has been recently tried by British practitioners; but owing to the uncertain purity of the preparations, the results have been less accordant than would otherwise have probably been the case. Dr. Clendinning reports very favourably of its sedative powers, con-

* *Edinburgh Medical and Surgical Journal*, October, 1843.

† *New York Journal of Medicine*, January, 1844.

‡ *New York Journal of Medicine*, January, 1844.

sidering it to possess all the virtues, without the inconveniences, of opium, and says,* that with remarkably few exceptions it has acted as a soporific in conciliating sleep, as an anodyne in lulling irritation, as an anti-spasmodic in checking cough and cramp, and as a nervine stimulant in removing languor and raising the pulse and spirits. These effects followed its employment in both acute and chronic disease. The usual dose was half a drachm of the tincture.

Dr. J. C. Badely, of Chelmsford,† has extensively tried the tincture and extract of the *Cynara* (artichoke) in rheumatism and neuralgic affections, and is quite satisfied of its value as an anodyne. In gout, he considers it as almost a specific. A pure preparation may be obtained from Mr. Baker, of Chelmsford.

Camphor.—This well-known drug has found a most enthusiastic admirer in M. Raspail,‡ who, for five years, has been in the habit of using it in every possible form, internally and externally. "I have entered," he says, "upon a new kind of existence; I have, so to speak, shed the old skin of disease; I have grown young again in physical and in moral strength; I am more disposed to labour, and am less inconvenienced than ever by it. I, therefore, consider myself justified in recommending to others to partake of the benefits derived from this long and conclusive trial." How singularly combined are strength and weakness, even in the best of men. If permitted to enlarge the boundaries of human knowledge, they prove their participation in human credulity.

Tonics—Quinine.—Dr. Melier,§ in conjunction with Magendie, has investigated the poisonous properties of quinine, and they have found in animals, congestion of the lungs and deficient coagulability of the blood to be the principal *post-mortem* effects. Its action was much more energetic when given fasting and in acid solution; its absorption and elimination were rapid.

It is stated that fifteen grains of quinine, given in the infusion of senna, is more efficacious as a tonic, notwithstanding the purgative quality of the mixture, than nearly double the quantity administered in the form of pills.||

* *Transactions of the Medico-Chirurgical Society*, vol. viii.

† *Lancet*, July 15, 1843.

‡ *Medical Times*, November 25, 1843.

§ *Memoires de l'Academie Royale de la Medecine*, tome x., 1843.

|| *Lancet*, November 4, 1843.

After comparative trials as to the best mode of administering quinine in quotidian ague, Dr. Stratton gives the preference to a single large dose shortly after the paroxysm.*

The *malambo*, or matias bark, first described by Dr. Mackay, is favourably mentioned by Dr. Ure,† as combining the advantages of a tonic and aromatic. It is also compatible with salts of mercury and iron.

Dr. Branson, of Sheffield,‡ has observed a similar blue line upon the gums, in patients taking the nitrate of silver, to that described by Dr. Burton in the case of lead. He considers it a good indication of the extent to which we may administer the medicine without fear of discoloration. The fact, also, is interesting, as pointing out a common principle of action in mineral remedies.

Strychnia.—From some experiments on cold-blooded animals, made at Heidelberg, by Dr. Pickford,§ it has been concluded that the effect of strychnia is to augment considerably the reflex activity of the central nerves, voluntary and involuntary, but it destroys the reciprocal action which the two systems of reflexion undergo in the medulla oblongata.

A very interesting series of experiments on rabbits, dogs, and guinea pigs, has been instituted by M. Dudos,|| who found, after poisoning these animals with strychnine and brucine, that they were restored by negative, while death was hastened by positive electricity. These facts are well worthy of renewed investigation.

Dr. Douglas Maclagan has directed attention to bebeerine and its salts, as substitutes for quinine. They possess the advantages of cheapness and not disturbing the action of the brain.¶

Astringents—Ergotine.—The extract of the secale, prepared by M. Bonjean,** is regarded by him as a specific in all cases of hæmorrhage. M. Anal has, he says, treated thirty-six cases of uterine engorgement with the same preparation, continuing its

* *Edinburgh Medical and Surgical Journal*, May, 1844.
† *Pharmaceutical Society*, October 1, 1843.
‡ *Provincial Medical Journal*, January 19, 1844.
§ *Gazette Medicale de Paris*, Janvier 18, 1844.
|| *Buchner's Rep. fur die Pharm.*, vol. xxxii.
¶ *London and Edinburgh Monthly Journal*, August, 1843.
** *Bouchardat, Annuaire de Therapeutique*, 1844.

use for months without inconvenience. He also recommends it in cases of uterine ulceration, chlorosis and leucorrhœa. The poisonous part of the ergot is supposed to be removed.

Dr. Beatty,* in a very practical paper, directs attention to the poisonous action of secale upon the child, which presents a peculiar livid appearance, with universal muscular rigidity, followed by a singular alternating form of spasm in those cases which recovered. His practical deduction is, that the ergot should never be given where there is a likelihood of the labour lasting more than two hours after its administration, and that if delayed beyond this period, we should resort to artificial delivery.

An extract of *ratanhy*, prepared by digesting it in sulphuric ether, has been employed by M. Tissier,† of Lyons, in passive hæmorrhages and mucous discharges, with great success.

The astringent powers of *matico*, first introduced to the profession by Dr. Jeffreys, of Liverpool, are confirmed by Dr. Hunter Lane and other observers. Dr. Lane also thinks very favourably of the extract of *monesia*, as prepared by M. Pelletier.

Hydriodate of Potass.—The therapeutic employment of this important remedy continues the subject of renewed investigations. Dr. Oke,‡ of Southampton, in a very practical communication, considers it useless in cases of abdominal tumours, scirrhous induration and ulceration, lupus, tubercles of the glands and lungs, enlargement of the liver, ascites, and encysted dropsy. On the other hand, he confirms its great value in phagedenic ulceration, periosteal disease, and other forms of syphilitic cachexia, and in chronic rheumatism. He also relates some cases illustrative of its use in certain forms of dyspepsia, apyrexial asthma, and chorea. Such statements of personal experience will, we hope, find many imitators.

Dr. Vincenz Roederer§ has for the last five years watched the constitutional effects of this drug in a large number of persons, who have systematically taken it for syphilitic affections, and thinks to have observed, that although the disease has been cured, the

* *Dublin Journal of Medical Science*, May, 1844.

† *Gazette des Hôpitaux*, Juin 6, 1843.

‡ *Provincial Medical and Surgical Journal*, April 24, and May 1, 1844.

§ *Oesterreich. Medicinische Wochenschrift.*

patient presents a peculiar leaden discoloration of the skin, a haggard expression of countenance, with general emaciation, more particularly of the face, neck, and hips. A steady use of the vapour bath is strongly recommended under these circumstances. Dr. Roëderer also considers the hydriodate as very injurious in scrofulous cachexia, and quite inert in cancerous and tubercular affections.

In the *London and Edinburgh Monthly Journal* for May, 1844, Dr. Hocken has entered into an elaborate comparative analysis of the relative therapeutic value of the iodide of potassium and mercury, and concludes that it is against the tertiary symptoms that the effects of iodine are decided and certain, while it is almost negative in the primary stage, and principally valuable against secondary symptoms, when associated with a cachectic and debilitated constitution.

Dr. Taylor,* of New York, has derived much advantage from Mr. Donovan's triple combination of iodine, arsenic, and mercury, in many forms of cutaneous and uterine disease.

A memoir has been presented to the French Academy by MM. Guillot and Melseus, on the curative power of the hydriodate of potass in saturnine diseases. The dose is gradually increased, from half a drachm to two drachms a day. From six to ten ounces have appeared sufficient to complete the treatment.

Dr. Lanyon† relates twenty-two cases consisting of cutaneous disease, erysipelas, and swelled joints, all yielding to the external application of a strong iodine tincture.

Tartar Emetic.—Antimony, like mercury, is one of those medicines, the effects of which are very various, according to the idiosyncracies of patients and the state of the system at the period of administration. Mr. Noble,‡ of Manchester, states that in children, even in small doses, he believes it to be not an unfrequent cause of sudden death. He confirms his own experience by that of Mr. Goodlad and others, and cases have since been related tending to the same conclusion, so that the question is one well deserving the practitioner's serious consideration. As a general rule, it would be far

* *American Journal of the Medical Sciences*, April, 1844.

† *Lancet*, August 12, 1843.

‡ *Provincial Medical and Surgical Journal*, April 24, 1844.

better to relinquish the use of all violent and depressing remedies against the diseases of tender infancy.

Mr. Trix,* chemist, of Exeter, has introduced a new preparation of antimony, which Mr. Wardleworth prefers to every other form of this mineral. In doses of three to five grains every four or six hours, it is powerfully diuretic; and from its not nauseating, he thinks it well adapted to children.

Taraxacum.—The cream of taraxacum, obtained as recommended by Dr. Collier,† is, probably, the most effective, as well as the most simple preparation of this fashionable medicine.

Turpentine.—The following electuary is said to combine the virtues, without the disgusts of turpentine:‡—Take of gum acacia, ℈x.; mix with ℈x. of water; add of white honey, ℈l., and an equal quantity of oil of turpentine, with a sufficiency of magnesia to form a soft electuary. Dose ʒss., ʒij. daily, in unleavened bread.

The *chloride of magnesium* is recommended by Dr. Lebert§ as an agreeable, efficacious, and useful neutral aperient salt.

FORENSIC MEDICINE.

Medical Coroners.—The proceedings which have recently taken place at a coroner's inquest, in a case of probable poisoning of an infant by opium,‖ where the strong statements of the medical witness were rejected by the coroner, upon grounds which the merest tyro in medicine would have known to be erroneous and insufficient—this, with other analogous and often recurring facts, have too clearly indicated the anomaly and mischief of entrusting to unqualified individuals investigations and decisions which can only be safely undertaken by the educated medical practitioner.

Where the causes of death are to be carefully examined, and where the life and reputation of individuals depend upon the knowledge of those who conduct the inquiry, it does, indeed, seem

* *Medical Times*, October 28, 1843.
† *Lancet*, September 26, 1843.
‡ *Bulletin Generale de Therapeutique.*
§ *Archives Generales de Medecine*, Mars, 1844.
‖ *Provincial Medical Journal*, July, 1843.

only consonant with common sense that such duties should be restricted to those who are fitted from their acquirements to fulfil them. But this apparently self-evident axiom is so far from being practically admitted, that it is only within the last few years, and that mainly through the zealous efforts of Mr. Wakley, that these strictly professional duties have ever been entrusted to medical men.

Whatever legal difficulties may present themselves, they should, of course, be referred to those best qualified to solve them; but that sound medical knowledge is the chief requisite for a coroner, in the vast majority of instances, no one who is familiar with the details of such proceedings can reasonably deny. It is, therefore, the duty of the profession fearlessly to urge this question upon the attention both of the Government and of the public, and for medical men to come forward as candidates for the office whenever vacancies may occur. Were all the evidence at coroner's inquests correctly reported by medical observers, for a single year, it would constitute a chapter of no little interest and instruction.

Arsenic.—The tests and antidotes for arsenic are still undergoing varied and careful experimentation.

The method proposed by Professor Reinsch, at the close of the year 1842, has been rendered more certain and complete by Professor Christison,* who advises the copper on which the arsenic is deposited to be cut into small chips, packed into a glass tube, and a slow red heat applied. A white crystalline powder sublimes, which, when examined in the sunshine, or with a candle near it, under a magnifier, of four or five powers, will enable the observer to distinguish the equilateral triangles composing the facettes of the octaëdral crystals of arsenious acid. So modified, no test, says Dr. Christison, is equal to Reinsch's in facility, delicacy, or conclusiveness.

The fact that the reduction of metals may be easily effected by causing an electric current to pass through a metallic rod, one end of which is immersed in a dilute acid, and the other in a solution of its salts, has been applied by Dr. Bevan† to the detection of arsenic, who considers it a certain mode of determining

* *London and Edinburgh Monthly Journal*, September, 1843.

† *Medical Times*, September 2, 1843.

the presence or absence of this mineral in the suspected fluid. A similar fact has been stated by Hugo Keich in Germany.

Mr. Robert Ellis,* a student of University College, in a highly creditable communication, has suggested a simplification of Marsh's process, founded upon the observation, that arseniuretted hydrogen deposits its arsenic upon copper wire at a moderate temperature; and in a second essay, dated December 23rd, he mentions a still further improvement, produced by mixing the suspected fluid with sulphuric acid, water, and zinc, and passing a slow current of the generated arseniuretted hydrogen over the black or red oxyde of copper, enclosed in a tube, when the whole of the arsenic combines with the copper, and by heating the compound in a bent tube, brilliant clusters of crystals of arsenious acid may be easily obtained. This property of the oxydes of copper has not before been so applied, and Mr. Ellis's process seems also well adapted to determine the whole quantity of arsenic present.

Dr. Shearman,† in a very practical paper on the necessary tests and antidotes for arsenic, after pointing out the objections to Marsh's apparatus and others, where zinc is employed for the production of hydrogen, gives the preference to the decomposition of distilled water by galvanism, to which the suspected solution is added with pure sulphuric acid, collecting the hydrogen from the negative pole or zincode of Smee's battery, igniting it, and examining the stain left in a glass tube open at both ends. If there is the smallest particle of arsenic, the hydrogen will combine with it, and the arsenical crystals be formed.

Dr. Shearman has successfully modified Mr. Ellis's test by the above plan, and freed it from the objection of using zinc.

Dr. Favell suggests that in cases likely to be followed by legal investigation, the sulphate of zinc should never be employed as an emetic; nor should the stomach-pump be used, after the arsenic has had time to act upon the mucous membrane, for fear of perforation.

Some fresh and satisfactory experiments have been made by Dr. Shearman on dogs and rabbits, to test the antidotal power of the hydrated peroxyde of iron, and MM. Bouchardat and Sandras‡ state that the hydrated persulphuret of iron has peculiar claims to

* *Lancet*, November 11, 1843.

† *Provincial Medical and Surgical Journal*, April 3, 1844.

‡ *Bouchardat Annuaire de Therapeutique*, 1844.

attention, because equally adapted to the salts of mercury, arsenic, copper, and lead. This preparation should therefore be constantly kept in readiness by all chemists and druggists.

We would refer to some able papers on the detection and quantitative determination of arsenic by Dr. Fresenius, now publishing in the *Medical Times*, and also to Mr. Taylor's admirable *Manual of Legal Medicine*, where the important subject is very fully and ably investigated.

Several cases of poisoning by arsenic have been reported during the past year, in two of which the free administration of the hydrated peroxyde of iron was attended with very gratifying success. Dr. G. Bird has mentioned that it may be extemporaneously prepared by adding an ounce of the liquor potassæ to half an ounce of the muriated tincture of iron.

An interesting case of slow poisoning from the use of Fowler's solution, in doses augmented from five to fifteen drops three times a day for a month, is related by Mr. Jones, of Strefford.* The patient very slowly recovered.

Raspail† very strongly objects to the medicinal use of arsenic in any form or dose, as inflicting lasting injury upon the system, and states that a knowledge of these slow but injurious effects, caused its use to be abandoned more than a hundred years ago in Germany. The celebrated Aquetta di Napoli was nothing more than common water holding in solution a small quantity of arsenious acid.

The same observer lays much stress upon two negative symptoms as indicative of poisoning by arsenic. In a strong dose it destroys life in twelve hours at the farthest; and secondly, the vomiting is never accompanied with stercoraceous matter. After looking over more than two hundred volumes of medical journals he could find no exceptional instance.

Corrosive Sublimate.—Dr. Frampton‡ has proposed the use of silver as a test, upon the grounds that the affinity of silver for chlorine, and its great tendency to amalgamate with mercury, offer facilities for detection superior to all other methods. In reply to

* *Provincial Medical Journal*, November 18, 1843.

† *Medical Times*, November 4, 1843.

‡ *Medical Gazette*, August 4, 11, and 20, 1843.

a correspondent, who gives the preference to tin, Dr. Frampton proves the extreme sensitiveness of silver for mercury in vegeto-organic compounds, detecting one part in 180,000, while neither Devergie nor Orfila claim more than 1 in 80,000 for either the proto-chlorides of tin or copper.

Two cases of poisoning by this salt are related by Mr. Morris, of Spalding,* and Dr. James Watson, of Glasgow.† In the former nearly half a drachm was swallowed, and in addition to the ordinary local symptoms and the drowsiness mentioned by Dr. Christison, there was a remarkable slowness and intermittence of the pulse, which was not more than 30 in the minute. In Dr. Watson's case, although a drachm was swallowed, the patient survived seven days, and no real salivation took place. No trace of the poison could be detected in the intestinal canal after death, but there was an inflammatory state of the œsophagus, left half of stomach, the lower part of the ileum, colon, and especially of the rectum. The liver and the other viscera were not tested.

Lead.—A very elaborate memoir on poisoning from lead was presented to the French Academy by MM. Flandin and Danger on the 29th of January, 1844. The following is a summary of their more important conclusions:—There exists no lead in the human body in the normal state. The symptoms and *post-mortem* appearances produced by lead have a peculiar character. If death has been caused immediately by this poison it is found in the body as surely as arsenic, antimony, and copper. It should be particularly sought for in the intestines, liver, spleen, kidneys, and lungs. It is not to be detected in the blood, nor yet in the heart, brain, muscles, or bones. As it only exists in the proportion of one-hundred-thousandth part, at least from fifty to sixty grammes of the liver must be tested. The best mode of operating consists in carbonising the animal matter by means of sulphuric acid, heating the carbon to redness, and then submitting it to the action of muriatic acid and water, in order to employ the different tests. Lead, unlike copper, is eliminated by the kidneys. Metallic poisons are principally absorbed by the vena portæ, hence they are found so abundantly in the liver. When poisons have been absorbed by

* *Provincial Medical Journal*, November 18, 1843.

† *London and Edinburgh Monthly Journal*, May, 1844.

the skin, they may be eliminated in the digestive tube, and rejected by vomit or stool, or may then be absorbed into the portal vein, as if originally introduced into the stomach. In cases of medico-legal investigation the liver ought to be selected for experiments, about a tenth part of that organ being first operated upon.

Professor Cozzi has detected the presence of a salt and oxide of lead in the blood of a man suffering from lead colic, and ascertained that they are only combined with the albuminous principle. This analysis, which supports the theories of Berzelius, Lassaigne, and others, is the first by which we have been made acquainted with which of the proximate principles of the blood lead really enters into combination. It had been previously established by Chevalier and Bricheteau, that the urine of white-lead makers contains this mineral.

An interesting example of slow poisoning occurred in an American family, from drinking cider poured into vessels newly painted with white lead and linseed oil.* No effect was observed for a fortnight. The chief peculiarity in the symptoms was a soreness in the soles of the feet, succeeded by slight nausea and a vesicular eruption. Dr. Shipman also adduces three cases of constitutional injury from the acetate of lead medicinally, but freely given. This remedy, he thinks, far too frequently and incautiously prescribed.

Dr. Hanle† mentions a case where an individual suffered severely from drinking some liqueur from a bottle where some shots, used for cleaning, had been left.

Copper.—The conclusions of MM. Flandin and Danger‡ in regard to copper are very similar to those already cited in the section on lead. They deny its existence in the body in its normal state, contrary to the opinion of Orfila and others. Unlike lead, it is not eliminated by the kidneys, but by the bronchial membrane and the bowels, and in accordance with this fact, the treatment recommended consists of emetics combined with steel filings and sulphuric lemonade, followed by sudorifics and emeto-cathartics. After death it is principally found in the liver, and from one to two ounces of this organ is sufficient to demonstrate its presence.

* *American Journal of the Medical Sciences*, July, 1843.

† *Journal de Chimie.*

‡ *Medical Times*, January 6 and 13, 1844.

The now ascertained localisation of poisons in the viscera, will prevent the possibility of confounding the effects of imbibition after death with those of absorption during life.

Dr. Beer, of Vienna, relates five cases of children suffering from blue and green sweetmeats: copper appeared to be the deleterious agent.

Sulphate of Iron.—The experiments of Smith and Orfila have proved this salt to be an irritant poison of no small energy. An instance, where it caused death, was related before the Edinburgh Medico-Chirurgical Society, April 3, 1844.

Bi-chromate of Potash.—Dr. Wilson* relates the case of an old man, who died from a large dose of the bi-chromate of potash. The effects were those of a sedative character; it neither excited vomiting nor diarrhœa.

Prussic Acid.—Accurate acquaintance with the physiological action of medicines manifestly contributes to their rational therapeutic employment, and is in proportion important as the agent is capable of injurious effects.

A series of experiments on rabbits, pigeons, lizards, frogs, and fishes, with Ithner's prussic acid, has been made by Dr. H. Megrier,† and he concludes that its local action on the nervous extremities is to suppress motion and sensation, and to cause congestion and increased secretion. It only acts upon the system when received into the blood, and death arises from paralysis of the heart. This paralysis of the heart is almost instantaneous when the acid is introduced into the lungs. Contrary to the usual opinion, the blood in every instance coagulated after death. The principal general symptoms were tetanic phenomena and distension of the venous system, with serous exudation. These results explain the *modus operandi* of this singular agent in cases of neuralgia and cardiac palpitation.

Cherry Laurel Water.—There are few opportunities of watching the effects of poisonous doses of prussic acid in man. Dr. C. Kurhn relates a case where an ounce and a half of the *Aqua Lauro-cerasi* was taken. After three hours there was general motor-paralysis of all the voluntary muscles, with loss of temperature and feeble

* *Medical Gazette*, March 9, 1844.

† *Medical Times*, March 16, 1844.

pulse; mental consciousness was unimpaired. Death seemed the consequence of paralysis of the lungs. The blood, after death, presented a remarkably dark and greasy character, but no smell of bitter almonds was perceptible.

Five French soldiers were seriously affected from having partaken of soup which had been stirred with a branch of the laurel rose.* (*Nerium Oleander.*)

Œnanthe crocata.—Some roots of this plant were eaten, in mistake for celery, by some convicts at Woolwich.† Poisonous effects rapidly followed, consisting of violent irritation of the mucous membrane of the œsophagus and stomach, with severe muscular spasms, delirium, coma, insensibility, extreme congestion of internal organs, and, in those cases immediately fatal, the blood was permanently fluid. Considering the great activity of this poison, that it is capable of extinguishing the life of a strong man in one hour, it is rather singular that the nature of the active principle of the œnanthe is not known, or the plant applied to medicinal use. Mr. P. Bossey advises the chief reliance to be placed on emetics, large blood-letting, and the cold affusion.

Cytisus Laburnum.—Professor Christison‡ relates a singular case of poisoning by the bark of the laburnum tree; and from experiments on rabbits, its action appears to be very energetic. Professor Christison ranks it among the narcotico-irritants. Such a fact is not without importance, when relating to a substance so common and so easily obtained.

Croton Oil.—Cases of poisoning by croton oil are rare. A teaspoonful was administered, by mistake, to a child, four years old,§ which had previously eaten a hearty meal of bread and milk. In five minutes it was seized with violent vomiting and purging, soon followed by alarming prostration. By the use of warm fomentations and free libations of milk and mucilage, it was convalescent in two days.

We have known very similar symptoms follow the administration of half a drop in a lady suffering from severe neuralgia.

* *Journal de Chimie*, Septembre, 1844.

† *Medical Gazette*, May 31, 1844.

‡ *Edinburgh Medical and Surgical Journal*, October, 1843.

§ *London and Edinburgh Monthly Journal*, November, 1843.

Aconite.—Dr. Francis Devay,* of Lyons, reports a case where ten drachms of the tincture of aconite was swallowed by mistake. The most violent cerebro-spinal symptoms succeeded for some hours, after which the patient gradually recovered.

Sulphate of Potass.—A good deal of discussion on the poisonous effects of the sulphate of potass has taken place during the past year;† and though originally founded upon a doubtful fact, sufficient evidence has been elicited to prove that this salt exerts a very irritating action under certain circumstances, and in large doses may prove fatal. The recent experiments of Bouchardat on the action of metallic and neutral salts on vegetable and animal life, are in favour of such a fact. He found that the sulphate of potass differed much from the sulphates of soda and magnesia. Fishes quickly perish in a solution containing $\frac{1}{100}$th part of the salt, while they exist forty-eight hours in water containing $\frac{1}{20}$th of the others. Plants are also more quickly and injuriously affected. Its spiculated form, or its adulterations with zinc or arsenic, have been suggested by others as the causes of its virulency.

Hæmorrhage.—A curious case of sudden and suspicious death of a pregnant female is related by Mr. Thursfield, of Kidderminster.‡ It depended on the rupture of a varix in the vagina, from a slight and unintentional injury. Dr. Meadows§ also mentions a remarkable instance of fatal hæmorrhage from the right ovary, occurring under circumstances where poison was suspected.

Alcohol.—A fatal case is recorded by Dr. Nicol,‖ where the intense congestion of the œsophagus, stomach, and intestines, led to the suspicion of some corrosive poison, but which was proved to have been produced by whiskey. Dr. Nicol remarks, that in the stomach of persons dying from arsenic, the inflamed patches were coated with lymph, and limited to the prominent parts of the plicæ, while from alcohol the congestion is general and without lymph. Dr. Yellowly's very able analysis of the appearance of the

* *Medical Times*, January 20, 1844.
† *Medical Gazette*, October 27, 1843.
‡ *Provincial Medical Journal*, October 14, 1843.
§ *Lancet*, May 4, 1844.
‖ *London and Edinburgh Monthly Journal*, 1844.

stomach after death from various causes, demonstrates the uncertain value of mere vascularity, either as a sign of disease or poison.

Abortion.—In consequence of a recent trial in Exeter,* Dr. Shapter has investigated the evidence which exists as to the power of particular medicines in inducing premature fœtal expulsion. His researches are limited to *Savin, Cantharides,* and *Secale,* and from the data he has been enabled to collect he concludes,—that neither of the two former are entitled to be classed as specific exciters of uterine action, and that their exhibition for the purpose of inducing abortion should never be entertained; that there are no medicines known, with the exception of the ergot, capable of exerting such a power; that *Secale,* when so administered, is uncertain in its effects, and most probably acts only during the later periods of pregnancy; that both in a professional and scientific point of view, it is highly injudicious to exhibit the ergot of rye with the intention of procuring abortion.

Dr. Shapter's statements have been partially assailed by one or two succeeding writers, and the question may be regarded as still open to discussion. The record of opinions on the value and uses of ergot, if only limited to the past year, would furnish some very singular evidence of the conflicting uncertainties of human observation.

MEDICAL REFORM—EMPIRICISM.

We do not consider it our province to enter upon a detail of what has taken place since we last met on the subject of medical reform, but we much regret that in the measures contemplated by Government, there is no indication of any intention actively to legislate against the unqualified practitioner, or to suppress or regulate the sale of empirical nostrums. We confess ourselves weary with the effort to rouse a feeling of bold resistance to quackery in every form, and to popularise the conviction that it ought to be restrained by the strong arm of the law. On what principle law is to prove inoperative for such a purpose we have never been able to comprehend; and to expect the remedy in the diffusion of knowledge, or in the good sense of those who never

* *Provincial Medical and Surgical Journal,* April 10 and 17, 1844.

can and never will appreciate the true merits of the question, we can only regard as a vain and hopeless chimera.

At all events we would advocate the principle, that the ban of illegality should be attached to unqualified practice; that all Government participation and protection should be withdrawn; that the legalised practitioner should, at least, be outwardly honoured; that the press should not be a sanctioned vehicle for announcements which are palpably false and necessarily injurious; that men who are manifestly disregarding the first principles of truth, and making merchandise of public ignorance and credulity, should not be assisted in the diffusion of falsehood by agents intended only to benefit and enlighten. If quackery cannot be so extirpated, and we admit the fact, shroud it as far as possible in its native darkness and obscurity; deprive it of those channels of circulation which are now so unsparingly afforded; attach to it the stigma of violating national regulations; submit it to severe and easily imposed penalties; and draw the broadest possible line between the man who obeys and the man who infringes the established institutions of his country.

That restrictions so imposed would admit of frequent evasion, no one acquainted with human nature can reasonably doubt; but that they would materially diminish the enormous extent of evil now inflicted with impunity, upon the public health, we believe to be equally capable of demonstration. As a profession, we should at least occupy the outward position we are entitled to hold, and the Government would have done what the governed have a right to demand.

We are aware that the authority of great names is against all such interference; but time has only strengthened our convictions of the practicability and truth of early recommendations, and it therefore becomes a duty, at such a moment as the present, again to urge them upon the attention of this great Association.

While making these remarks we would not forget, that the true elements of medical reform are rather personal than corporate; that a high standard of individual conduct must be adopted if, as a body, we would be purified; and that whatever delays may attend the introduction of legislative reform, there is a power entrusted to every one of us of *internal* reformation; of a sound and healthy training of the moral and intellectual faculties; of diligence in the

pursuit of science; of modesty in the self-estimation of attainments; of moderation in all desires for mere applause or emolument; of refusal to stoop to what is selfish and degrading; of rightly estimating the holy luxury of doing good, a power far more important than the best regulated charters, the true and only lasting element of our individual or collective prosperity. It has been truly said, that a profession, every member of which had these qualities in abundance, might indeed have quacks for its rivals, but would triumph without a combat.

To Dr. Hastings the profession owes its best thanks, for his bold and fearless expositions of the dangers and abuses of hydropathy.* Would that all medical men were equally willing to denounce the evils of blind empiricism, and that they would shake off that hyper-sensitiveness to popular prejudice and to their own interest, which makes them shrink from a warfare they are morally bound to undertake.

To Mr. Estlin, of Bristol, and, still more recently, to Dr. Forbes, we have to express our obligations, for their having zealously and manfully exerted themselves to arrest and dissipate a delusion which is setting common sense at defiance, and subverting the first principles of a sound philosophy. It is deeply to be regretted that many members of the profession have given an incautious and precipitate sanction to the seeming wonders, but real juggleries, of *animal magnetism*, and have united with a credulous public in attesting the truth of phenomena, which carefully conducted experiments have over and over again demonstrated to be delusive. By such hasty acquiescence in the apparent results of investigations, which, with few exceptions, have been conducted in a manner incapable of establishing truth, error has been perpetuated, and medical men have descended from the situation they ought ever to occupy. The whole question is one which should never have been submitted to a popular tribunal, requiring, as it does, no ordinary sagacity and care for its scientific examination, and involving results far better concealed from, than exposed to the prurient gaze of popular curiosity.

A more profound acquaintance with the previous history and progress of animal magnetism, and a deeper insight into the sources and circumstances of human delusion, with a just estimate

* *Provincial Medical Journal*, July 22, 1843.

of the difficulties which beset all such inquiries, would have preserved many an honest and ardent mind from what will hereafter be regarded as a wild and singular infatuation.

To Dr. Alexander Wood, of Edinburgh, also, we are indebted for a masterly exposition of homœopathic absurdities.

BENEVOLENT FUND.

The detail and regulations of the "Benevolent Fund," being entrusted to the care of a committee, need not now occupy our time or attention. We may, however, be permitted to express a hope that this laudable branch of our proceedings may attain to a magnitude and an efficiency commensurate with the extent and importance of the Association. This will, we fear, never be the case while dependent upon voluntary contributions; and we still hope that it will be considered desirable, by the members generally and by the council, to devote a certain portion of a slightly increased annual subscription to so holy and unexceptionable a purpose. With our present numbers, a very moderate individual contribution would secure a steady and considerable income; and to engraft such an object into the constitution of the Association, would at once enlarge the sphere of our usefulness, and tend to perpetuate and strengthen the bonds of our union.

We cannot suppose that such an arrangement could prejudice in the slightest degree the interests of those excellent local benevolent societies already existing, while it would furnish means for aiding their limited resources, and providing for many distressing emergencies excluded from relief by the very nature of their rules.

No profession presents more numerous or more harrowing instances of pecuniary reverses and desolation than our own; while none is in possession of fewer means for cheering the bitter days of adversity. The philanthropic suggestion of Mr. Newnham, that we should form a large society for mutual insurance, deserves our serious and most attentive consideration;* and we trust that the day is not far distant when the victim of misfortune or disease, the bereaved widow, and the child of destitution, shall be sheltered from the direful necessity of dependence on the world's cold charity, and find in the ranks of a profession, ministering

* *Provincial Medical Journal*, September 23, 1843.

to all but to itself, that protection and sympathy to which its members have so natural and righteous a claim.

BIBLIOGRAPHY.

Amongst the more important additions to our medical literature, in the department more particularly included in the present address, we shall briefly allude to the following:—

To theoretical and practical medicine a valuable contribution has been made by Professor Williams. His volume, entitled *The Principles of Medicine,* is not merely a new book, but a work of no ordinary merit and importance, well worthy the attentive study of the practitioner as well as of the student. The author has done good service, by attempting, and we think as successfully as present circumstances permit, to point out the connexion between science and practical medicine. "It seems quite extraordinary," he observes, "notwithstanding the recent rapid improvements and comparative perfection of the contributary sciences, practical medicine should still halt in the domain of empiricism. A chief cause for the anomaly seems to be that science and practice have been rarely pursued by the same parties. Scientific men are not, and cannot be practical, because they have had no experience; and practitioners know little of science, and therefore derive little good from it. Instead of walking together, these parties are at issue with each other. But it is high time to put an end to this feud. Philosophers must descend from their transcendental position, to consider details of practice and purposes of utility. Those who would be practitioners must gain from science that knowledge and that method which renders experience instructive and useful." We cannot attempt any analysis of the volume before us. It would be easy to play the critic and find fault, but such a spirit we deprecate when noticing what as a whole is so intrinsically excellent. Dr. Williams deserves our best thanks for having reduced into a brief, but comprehensive statement, a subject of such difficulty and extent, evincing in the writer a combination of practical experience and scientific information with a soundness of judgment, which few are either qualified or privileged to attain.

The third and concluding part of Professor Alison's *Outlines of Pathology and Practice of Medicine,* including chronic or non-

febrile diseases, has been recently published. It displays the clear and methodised mind, as well as the extensive experience of its well-known and meritorious author; and the entire work may be regarded as a compendious summary of the present state of our knowledge of the pathology and treatment of disease. We think it rather adapted to refresh and prove suggestive to the mind of the practitioner, than to serve as a class book to the student. For the latter it is too succinct—too concentrated—requiring much previous information to profit by or estimate its comprehensive brevity.

The last part of the second volume of *Dr. Copland's Dictionary* has at length appeared. It evinces undiminished voracity of research, with equal care and lucidity of arrangement; and the whole work, when completed, will constitute one of the most gigantic monuments of individual labour which even the ponderous shelves of medical literature can boast.

We must not omit to mention the admirable essay of Mr. Travers, *On the Physiology of Inflammation and the Healing Process.* It contains a lucid and masterly summary of the present state of our information upon the question, conveyed in terse and elegant language, and is worthy of the author of "Constitutional Irritation." Mr. Travers combines great soundness and clearness of mind with extensive and accurate knowledge; he adopts no exclusive views, nor is he easily seduced by the speculative or new. There is a vein of wisdom and sobriety throughout, which renders his work well calculated to inform and to direct; and though leaving much that is unsatisfactory and unexplained, he has accomplished perhaps all that is either profitable or safe. "Inflammation (says the author) is not seated exclusively in the blood, blood-vessels, absorbents, or nerves of the part, nor confined to the capillary system of vessels, nor the cellular tissue which connects and supports them. These several parts forming a homogeneous whole, cannot be disjunctively affected in morbid more than in healthy action; each being an integral element in the composition of the other, reciprocating organisation and function, acting and acted upon respectively, their peculiar functions are merged in the production of *general* phenomena, whether healthy or morbid, to which each is indispensable." This extract will sufficiently illustrate the spirit of Mr. Travers's researches.

We hail with sincere satisfaction the appearance of the first volume of the Sydenham Society, not only on account of its intrinsic interest and value, but as the forerunner of a succession of healthy literature, which will at once elevate our taste, and reduce to their true level many of the trashy and ill-digested productions which are daily issuing from the press. At a moment, also, when we are so intensely occupied with ourselves, the diffusion of the labours of earlier and vigorous minds, is peculiarly refreshing; and a more familiar acquaintance with the sayings and doings of the past, cannot but be productive of a sounder estimate of our actual progress and merits.

Professor Hecker's *History of the Epidemics of the Middle Ages* is a valuable study to the physician, the philosopher, and the Christian. Records of remote and recent eras lie before us in rich abundance, and we trust that other and gifted minds will imitate the author, and mould them into forms of equal beauty and instruction. The translator and editor, Dr. Babington, deserves every praise, for the admirable manner in which he has accomplished his very laborious and responsible duties.

The second edition of M. Louis' admirable and well-known monograph on *Phthisis,* translated by Professor Walshe, has very quickly succeeded the above, and forms a most valuable and acceptable addition to our medical literature. The illustrious author has confirmed all his previous conclusions by a second series of observations, and given greater extension to the chapters on prognosis, diagnosis, etiology, and treatment. Chronic peritonitis, as an affection peculiar to phthisical subjects, as well as meningitis, has received a more minute and special examination, and the volume teems with undying evidence of patient labour, untiring zeal, and philosophic accuracy. It must ever be the text book on the pathology of consumption.

We are not fond of complaining on personal grounds, but as translators of the first edition, now nine years ago, to notice the existence of such a work, and to acknowledge such a fact, would, we conceive, on the part of Professor Walshe, have been consistent with professional courtesy and candour; at least, such, we trust, would have been our conduct towards himself, had our situations been reversed.

In the department of legal medicine, two excellent works have appeared. That by Mr. Taylor is rather an elaborate and complete treatise, than a manual, as he modestly terms it, and is among the few publications which realise more than they profess. It forms a most valuable contribution to this important section of medical investigation. The portion of the work devoted to poisons is peculiarly full and valuable, and arranged in a manner well calculated to guide the practitioner under circumstances of extreme anxiety and urgency. Mr. Taylor has proved himself a sound and an accomplished toxicologist.

Of the *Principles of Forensic Medicine,* by Professor Guy, the first and second parts are only yet published. The author has wisely abstained from overloading his subject with mere historical or literary detail, and has adapted his facts to our actual law and information. The important question of infanticide is very ably and minutely discussed, and some alterations in our present inefficient legal provisions are suggested. In the sections devoted to insanity, and particularly in the analysis of the plea of insanity in criminal cases, the reader will find much sound and valuable observation. It is rather singular that two works of such sterling excellence should have been published at the same moment.

New editions of the well-known works of Orfila and Beck have also appeared during the past year.

Memoires de la Société Médicale d'Observation de Paris, Tome deuxième. Though instituted more than six years ago, this is but the second volume of the society's transactions; a tardiness which ought not to surprise, when the high standard it adopts for all its communications is duly appreciated. Would that the flood of half-digested publications could be restrained by the same cautious spirit of inductive observation! How much vain labour would be spared, how many useless draughts upon the writer's and the reader's time avoided! and how much easier to dive after truth, amidst materials prepared for its elimination, than to seek it in the undistinguishable compound of fiction and reality, out of which it too often defies every effort at extrication! What a shrinking in the list of authorship would there be, were writers restricted in their wanderings to the laborious up-hill paths of rigid observation, and to an equally rigid induction from the facts they had accumulated! The example of the society, to whose transactions we have alluded, is one worthy of all imitation. To the labours

of no living member of the profession are we more deeply indebted than to those of the philosophic and industrious Louis. Perhaps this great Association cradles some ripening spirit who may hereafter be entitled to similar commendation!

M. Louis has for the first time printed the original of his admirable memoir on the yellow fever which prevailed at Gibraltar in 1828. It is already familiar to the English reader, by a translation published at Boston in 1839, by Dr. Shattuck.

Récherches et Observations sur les Causes des Maladies Scrofuleuses. Par J. G. A. Lugol, Medecin de l'Hôpital Saint Louis. This is a most important work, the result of twenty years' experience in hospital and private practice, and by a physician well qualified to avail himself of the remarkable facilities he possessed for the investigation and treatment of scrofula. Dr. Lugol has not written a book to obtain practice, but has embodied the well-considered experience of long years of careful study and unexampled opportunities, for the benefit and guidance of others. It is the first part only of this excellent monograph which is now published. It cannot fail to replace all minor treatises upon the subject, and to exert a most beneficial influence, by diffusing correct ideas of the pathology and therapeutics of strumous disease. We are glad to be able to state that an English translation, by Dr. Ranking, is in course of publication.

Amidst a host of minor works or re-publications worthy of attention, we may mention:—

Dr. Watson's excellent *Lectures on the Principles and Practice of Physic,* now published in a separate form.

A Practical Manual on Diseases of the Heart and Great Vessels, by M. Aran, of Paris, and translated by Dr. Harris, of Philadelphia.

Two works on *The Tongue as an Indication of Disease,* by Dr. Benjamin Ridge and Dr. Edward Williams.

Dr. Marshall Hall's new *Memoir on the Nervous System,* embodying his physiological and practical views.

The excellent treatise on the *Nature and Treatment of Tic Douloureux,* by Dr. Henry Hunt.

An essay on *Spinal Irritation,* by Dr. Riadore; and Mr. Fletcher's *Sketches from the Case Book,* illustrative of the treatment of some important brain and nervous disturbances.

Mr. Erasmus Wilson's volume on *Diseases of the Skin* is the product of a practical, clear-thinking mind; and *Organic Diseases of the Womb* have been successfully investigated in the treatise of Dr. Lever.

The Conspectus of the Three British Pharmacopœias, by Dr. Neligan, is an able and useful publication; and a convenient assistant to the study of Organic Chemistry is presented in the *Practical Manual* of Dr. Griffith.

The admirable work on the *Theory and Practice of Ventilation,* by Dr. Reid, is well worthy the attention and study of every member of the profession. It breathes the spirit of the philanthropist as well as of the philosopher.

A second edition of the *Registrar's Report,* for 1843, has been recently published, containing an immense mass of most valuable and well-arranged statistical information. Mr. Farr's *Medical Reports* are worthy of the highest commendation.

In Mr. Chadwick's supplement, on *The Practice of Interment in Towns,* everything has been said and done which could elucidate this important and interesting subject.

Our periodical literature has continued to advance both in extent and activity. Limiting our observations to Great Britain, we would simply remark that the north of Scotland now boasts a journal of its own; and that the conductors of the *Medical Times,* and, more recently, of the *Lancet,* while reducing the price, have given to those journals an extent and importance far beyond what any weekly publication has hitherto attained.

Our own *Journal,* though yet very far from expressing the strength of the Association, has not been deficient in much valuable original matter, and is entitled to the high credit of exemption from all editorial abuse and personal altercations. A much smaller number of medical periodicals, if truthfully and energetically conducted, might be most advantageously substituted for their, at present, most inconvenient multiplication.

OBITUARY.

We must now turn aside for a brief moment from the records of a living acting world, and recall the names and memory of those, who, since our last meeting together, have ceased to be numbered

among its busy inhabitants; who, by their lives, have left us an example we should study and imitate, and by their deaths have bequeathed to us a warning we should never forget.

And, first, to anticipate the feeling of hearts now around me, I will mention the much honoured Dr. Barlow, who was removed by death on the 2nd of April, 1844, aged 65.

His life was too identified with this Association, his services are too recent, too important, and our sense of his sterling value and untiring benevolence, too sensitively acute, not to have our sympathies and regrets most painfully excited, by the reflection that he has passed away from amongst us for ever.

As one of the founders of the "Provincial Medical and Surgical Association," the author of our first Retrospective Address, in 1833, the promoter of every measure calculated to advance our collective or individual interests, Dr. Barlow has claims upon our respect and affections, of no ordinary character. He graduated in Edinburgh in 1803, and after three or four years practice as a surgeon in Dublin, he settled at Bath, as a physician, in 1807. He was elected to the Bath General Hospital in 1819, and to the City Infirmary in 1821; and on the junction of the latter charity with the Casualty Hospital, in 1826, he became senior physician to the United Hospital, both which offices he filled with an enlightened, self-sacrificing zeal, an assiduous attention to the duties of his station, and a humane consideration for the patients under his care, which entitle his memory to our highest estimation, and have embalmed him in the hearts of a poor, but grateful population.

His hospital labours were of a most onerous and unremitting character, and we doubt if any instance can be cited in the annals of a profession, abounding in examples of disinterested benevolence, where an individual has devoted a larger share of his time and energies to the gratuitous alleviation of human suffering.

A few brief words soon sum up the longest life; but, oh, how lengthened would be the catalogue, how incalculable the amount of those blessings, which are crowded into thirty-seven years of uninterrupted professional benevolence!

As an author, Dr. Barlow was both voluminous and successful. Besides very numerous and valuable contributions to our periodical literature, among the more important labours of his pen may be

mentioned his excellent "Essays and Reports on Medical Reform," a subject which, from the commencement of his career, engaged his warmest interest and attention. He was the author of eight excellent and well-known articles in the *Cyclopædia of Practical Medicine,* of which, the one on "Physical Education," has been translated into most of the continental languages.

In 1822, he published a very practical dissertation on *The Bath Waters;* and in 1825, a pamphlet entitled, *An Apology for the Study of Phrenology,* a science which he zealously, but rationally advocated. Of every measure affecting the moral or intellectual welfare of his fellow man, he was the willing and generous supporter, and his whole life was a continued manifestation of a vigorous and intensely active intellect, in union with an unselfish and benevolent spirit. As a friend—and we speak from much personal experience—his attachment was warm, deep, and disinterested; and though naturally reserved in the expression of his feelings, yet no one was ever more susceptible of the relief of unlimited confidence. His heart was too large and full to be ever on the lips, but it was often a well of sympathy and refreshment to those who needed the waters of encouragement in the hours of despondency and gloom. He was, upon principle and conviction, a firm believer in Christianity, and an attached and consistent member of the established church. Religion with him was not merely an intellectual abstraction, but the influencer of his heart and life. "It has long," he says, in a letter to the writer nearly two years before his death, "been the habit of my mind to regard each passing day as if it might prove my last; and to this habit no gloom is attached, but rather the contrary. I have felt grateful to an ever-merciful God for the blessings of life, but ready to resign them whenever it should be his will. At the age of sixty-three, my day of account cannot be far distant. I trust that I seek consolation where alone it can be found—in reliance on the promises of God, in unrepining submission to his will, and firm faith in the saving mercy of our blessed Redeemer's atonement." He now at length rests from his labours, but his works do follow him. It is for us not merely to grieve for his loss, but to emulate his worth, and strive to realise his hopes.

In the *Provincial Medical and Surgical Journal* for May 29, 1844, the reader will find a detailed list of Dr. Barlow's writings,

with some interesting observations from the pen of Mr. Soden, his intimate friend and associate.

The Association has also to mourn the loss, at the age of 73, of its much valued and venerable president, William Hey, Esq., of Leeds; and we much regret that it falls to the lot of one who was not privileged to be numbered among his acquaintances or friends, to refer to so painful a bereavement.

"Few men," says a correspondent, "have ever lived more truly beloved and respected, for his hand and heart were always prompt in every work of utility, benevolence, and religion, and his unobtrusive virtues and sterling qualities of piety and kindness in private life, secured him the esteem and affections of all who knew him."

Like his illustrious father, he had not to work his way into the position which he so successfully occupied, but was at once introduced into an extensive and well-established practice. He, however, fully maintained, through a long life, the great advantages he so fortunately inherited, and no one ever enjoyed, in a greater degree, the respect and esteem of their professional brethren.

But his career was by no means unclouded, nor sheltered from serious interruptions and trials. Very soon after commencing practice he sprained his ankle, from which he suffered so much inconvenience for nearly four years, that he hesitated as to relinquishing his professional pursuits. In 1811 he was severely injured by a fall from his gig, and in the following year he was attacked with hæmoptysis, followed by cough and excessive irritability of the trachea, which confined him to his room during the whole of the winter of 1812-13. He spent the following season in Devonshire, and it was not till the autumn of 1814 that he was enabled to resume his professional duties.

It was, we believe, about this period that he published his work on *Puerperal Fever*, the completion of which, illness had long delayed. A short time previously Dr. Armstrong's well known volume on the same subject, and advocating similar views, appeared, and Mr. Hey in his preface observes, that a reference to the dates of his cases, will show that his observations were prior to Dr. Armstrong's.

He was elected surgeon to the Leeds Infirmary in October, 1812, and retained that office until 1830, when he was succeeded by his

son, who is no unworthy, no degenerate representative of two ancestors "not more remarkable for professional eminence than for all the virtues which dignify the Christian citizen and adorn the private circle." We may also mention that he was many years an Alderman of the Borough of Leeds previously to the passing of the Municipal Reform Bill, that he twice served the office of Mayor, and that he was, up to the time of his death, a Magistrate for the West Riding of the County of York.

We need not recall, to those who were present, the winning courteousness and sound judgment which characterised his conduct when presiding over our anniversary proceedings during the past year. Though struggling with an enfeebled and sinking frame, the feelings of his heart were not the less sensitive to the associated interests of that profession which he had so sincerely loved and so successfully practised, and from which he then received that responsive and grateful expression of acknowledged respect, to which his long-tried worth and talents so peculiarly entitled him.

Another interval in our ranks has occurred from the death of Henry Rumsey, Esq., which took place at Gloucester, on the 23rd of November, 1843, aged 78. He was one of the oldest members of the College of Surgeons, and during fifty years an able and highly respected practitioner at Chesham, in Buckinghamshire.

Mr. Rumsey was a student at St. George's Hospital and the Hunterian School in 1785–6. His very diligent attention to the instructions of the great masters of that day was evidenced by the extraordinary number of their lectures, which he took down in short-hand, invariably copying his notes the same day. The learned editor of John Hunter's works selected for publication Mr. Rumsey's manuscript of the "Lectures on Surgery" as being the fullest and most accurate copy of any under examination.

Early classical pursuits gave a tone of refinement to his mind and a correctness of taste, which through life perceptibly influenced his manners and conversation.

Among the few medical papers which he left in print one deserves especial notice—"An Account of the Croup as it appeared in the Town and Neighbourhood of Chesham in 1793 and 1794." Dr. Baillie always referred to this paper in his Lectures, not merely as a faithful and vivid description of the disease, but as introducing

to the profession in this country the treatment of croup by calomel, first adopted by Dr. Rush, of Philadelphia.

Mr. Rumsey habitually took notes of all his more important cases, and recorded, in a tabular form, the particulars of nearly 3,000 midwifery attendances.

As a practitioner he was exemplary; humane, indefatigable, and cheerful, he was ever ready to obey the calls, whether from poor or rich, of a laborious and extensive country practice. His conduct was uniformly professional, gentleman-like, and liberal. Guileless himself, he scarcely ever suspected others; and always favourably interpreted the motives and actions of those who wronged him.

The first hour of his day, generally from six to seven o'clock, was devoted to religious exercises, and he managed so to arrange his numerous engagements as very rarely to omit attendance at public worship. His constant aim was to do good: oppression and injustice inflicted on any, and especially on the poor and helpless, excited his warm and active sympathy. Few private individuals felt more deeply, or exerted themselves more earnestly, in the struggle for the abolition of the slave trade. He was honoured by the personal friendship of Mr. Wilberforce, and during the latter part of that great and good man's life, had occasional opportunities of enjoying his society.

His kindness of disposition, his uprightness and purity of mind, and his entire freedom from selfishness, will never be forgotten by those who knew and honoured him.*

Another of our members, Mr. William Goodlad, Fellow of the Royal College of Surgeons, and one of the surgeons to the Union Hospital of Manchester, expired at his residence, Cheetham Hill, aged 65, on the 14th of February, 1844. He had practised for many years at Bury, and was held in much repute as an intelligent, talented, and active practitioner, and acquired considerable reputation as a skilful and bold operator. He was a surgeon of sound acquirements, of zealous industry in practice, and at all times a steady and ardent friend to the progress of knowledge and discovery. His professional character will be best estimated by an enumeration of his works.

An Essay on the Diseases of the Vessels and Glands of the Absorbent System, which obtained the prize offered by the London

* *Provincial Medical Journal.*

College of Surgeons, in 1814. Papers in the *Edinburgh Medical and Surgical Journal*:—On "The Origin of Calculi;" on "Purulent Ophthalmia;" on "The Treatment of Scrofula;" "A Case of Inguinal Aneurism, cured by tying the External Iliac Artery;" and in the seventh and eighth volumes of our *Transactions*, are papers describing a case of "Removal of a Large Tumour from the Face and Neck, in which the Common Carotid Artery was tied." He gained, we understand, the "Jacksonian Prize," in 1822; and his last paper was "A Critical Inquiry into Affections of the Spine," professedly in opposition to some views entertained by Sir Benjamin Brodie.

These are materials enough, says Mr. Stanley,* to show that, though Mr. Goodlad's sphere of observation was remote from the metropolis, his mind was equal to the highest efforts of surgery.

Sir Lachlan Maclean, M.D., of Sudbury, in Suffolk, one of the original members of the "Eastern Branch of the Provincial Medical and Surgical Association," has been removed, at the advanced age of 86. After making one or two voyages, in the capacity of surgeon, to the East Indies, he settled as a physician in the above borough, where he practised for half a century. The principal product of his pen was, *An Inquiry into the Cure of Hydrothorax*, dated 1810.

Two other members of the Association, both residing in Manchester, must be mentioned amongst those who cease to be numbered upon earth.

The history of Dr. Chaytor is that of many a noble and aspiring mind. He graduated in Edinburgh in 1833, and subsequently settled at Manchester. Earlier than usual he obtained the appointment of physician to two hospitals, and succeeded to the Chair of the Practice of Medicine, vacated by Dr. Bardsley, in 1842.

Of a naturally feeble constitution, his untiring application to his laborious clinical and other duties gradually undermined his health, and after several months of lingering illness, he died of consumption on the 1st of June, 1844. To an ardent and unremitting zeal in the pursuit of pathological investigations, and an entire devotedness to the practical duties of his profession, he added the finer feelings of morality and religion, and was

* *Medical Gazette*, March 8, 1844.

generally esteemed for the benevolence of his disposition and his unblemished professional character. It was at the meetings of this Association that I was permitted to renew the recollections of a friendship which, as fellow students and fellow graduates, we had formerly enjoyed.

Dr. Marshall was also a graduate of Edinburgh, in 1815; and after a short service as a military medical officer, he settled at Port Glasgow, whence he removed to Manchester ten or eleven years ago. He devoted much attention to diseases arising, or supposed to arise, from spinal irritation; and, in 1835, published a small volume on the subject.*

The profession has sustained other severe losses during the past year, to which, in conclusion, we must briefly refer.

Sir Henry Halford died on the 9th of March, in the 78th year of his age. Physician to four successive sovereigns, and consulted by many of the potentates of Europe, Sir Henry's professional life, from the commencement to the close of his career, brought him peculiarly in contact with the great and noble of the land. To considerable classical and literary attainments he united the polish and refinements of the accomplished gentleman.

A courtier by character and position, he was peculiarly fitted for the difficult situation he so long and so successfully maintained. To these qualities were united no inconsiderable professional sagacity and zeal. He presided for more than twenty years over the Royal College of Physicians, and was mainly instrumental in establishing the evening meetings. His well-known popular "Essays and Orations" are characterised by much sound sense, acute discrimination, and literary elegance of style. While his memory entitles him to our kind recollection and respect, he was too far removed from the necessity of toil to be reasonably expected to have been numbered amongst the great benefactors of our art.

The early death of Mr. Frederick Tyrrell is an event much to be deplored. "His life," says Mr. Stanley, "was one of incessant labour in the acquisition of scientific and practical knowledge; but it was the labour in which he delighted, urged on as he was by the conviction that it must be the only safe road to professional eminence and public favour."

* Proceedings of the "Newton Branch."

At the period of his premature death he was rapidly acquiring the confidence of his professional brethren, and rising in public estimation. He contributed two papers to the *Medico-Chirurgical Transactions:* one, "Remarks on Injury of the Head;" the other, "On the Treatment of Acute Purulent Ophthalmia;" but his principal contribution to medical literature was the elaborate work *On Diseases of the Eye,* a faithful record of more than twenty years' experience at the London Ophthalmic Hospital.

Dr. Burder, the author of *Letters from a Senior to a Junior Physician,* died at Tunbridge Wells, on the 16th of August, 1843, in his 54th year.* His life was distinguished by few incidents calculated to attract attention; but it presents a strong example of self-denying humanity, as well as of conscientious devotion to professional duty, which ought not to pass unrecorded. Dr. Burder studied medicine in Edinburgh with much assiduity, and in 1812 commenced practice in London; but, with a naturally susceptible constitution, his health soon proved unequal to the exertions which a steadily increasing professional reputation necessarily entailed. Dr. Burder's mental qualities were of a superior order. His accuracy in observing, and caution in deducing conclusions, are evinced in an interesting paper published by him in the *London Medical and Physical Journal,* for June, 1837, on the question of the "Cerebral Origin of Paraplegia;" and his articles on "Headache" and "Jaundice," in the *Cyclopædia of Practical Medicine,* possess no inconsiderable merit. His moral character was at once elevated and sensitive. Equally apt to applaud excellence, and disinclined to censure imperfection, he was never known to sully the fair fame of a competitor or to withhold a ready testimony to another's worth. His feeble health and ultimate retirement were mainly attributable to a sense of inability to realise the high estimate which he entertained of the duties of a physician; but, amidst conscious imperfections, his hopes rested on the cardinal doctrines of Christianity, which proved his solace in affliction and his confidence in death.

Dr. Abraham Colles, of Dublin, well known by his *Treatise on Syphilis,* who, during a period of thirty-four years, was professor of surgery in the Royal College of Surgeons, and for upwards of

* *British and Foreign Medical Review,* January, 1844.

twenty the acknowledged head of the surgical profession in Ireland, is among the list of those departed.

Dr. Colles was a singular example of the attainment of the highest rank and honour, without having excited the jealousy of a competitor, or made an enemy by his success. To an enthusiastic mind he united a nervous masculine understanding, which preserved him from every speculative delusion, and mainly contributed to the confidence he inspired and the solid reputation he enjoyed. He entertained a firm and sincere conviction of the great truths of Christianity, and it was in these that he found calm consolation in his last hours.

That indefatigable and enthusiastic physician, Dr. Chervin, has, after a life of hard toil and poor reward, been recently gathered to his fathers. His whole existence was devoted to the examination of the yellow fever. To discover the laws that regulate its origin and diffusion, to determine whether it be contagious or not, and to point out the sanitary regulations and precautions most necessary for the arrest of this destructive pestilence—such was the great object of all his labours. It was to the noble and elevated moral feeling that ever distinguished M. Chervin's career, his disinterested love of whatever is good and true, and his unquenchable and persevering devotedness of purpose, that we may attribute the universal regret that was manifested by all classes of the profession in France at the announcement of his death.*

M. Fournier, the author of a most meritorious and well-known work on *Diseases of the Chest,* has recently fallen a victim to consumption, when life's prospects were most fair and flattering.

Though not a member of the medical profession, we feel it our duty to allude to the loss we have sustained in Mr. Allen, the well-known and deservedly-respected chemist and druggist. In his early life he was a lecturer on chemistry at Guy's Hospital and the Royal Institution.

It is not for us to dwell on his persevering and philanthropic exertions in behalf of the injured sons of Africa, or on his usefulness as a member of the Society of Friends; but, regarding him as belonging to the important and influential body of druggists, we feel his memory to have a just claim to our grateful recollections, exerting himself as he did, through a long and active life, to

* *British and Foreign Medical Review*, January, 1844.

elevate the science, and to advance the respectability of his pharmaceutical brethren. To such men the medical profession will ever be most willing to acknowledge its obligations.

In conclusion, we may also mention the name of Professor Hope, who, for nearly half a century, held the Chair of Chemistry in Edinburgh, with no ordinary success and popularity; and, still more recently, we have to lament the loss of the celebrated Dalton, whose chemical discoveries have identified him with the science, and entitled him to the gratitude of his country.

RETROSPECT

OF

ANATOMY AND PHYSIOLOGY,

FOR THE YEAR 1843–44.

BY WILLIAM BUDD, M.D., EDIN.,

Physician to St. Peter's Hospital, Bristol;

DELIVERED AT THE TWELFTH ANNIVERSARY MEETING

OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION,

HELD AT NORTHAMPTON,

ON WEDNESDAY AND THURSDAY, AUGUST 7TH & 8TH, 1844.

The condition of physiology as a branch of knowledge—as an able writer has lately remarked of politics—was, up to a late period, that which Bacon animadverted upon as the natural state of the sciences, while their cultivation is abandoned to practitioners; not being carried on as a branch of speculative inquiry, but only with a view to the exigencies of daily practice, and the *experimenta fructifera*, therefore, being aimed at almost to the exclusion of the *lucifera*.

To this circumstance, quite as much as to the inherent and peculiar difficulties attending all investigation into the phenomena of life, must be ascribed the slow progress of physiology, while it was cultivated, more or less, with a view to its subservience to the practice of medicine.

There is, still, too general a disposition among medical practitioners to rate discoveries in this science in accordance with their immediate and obvious applications to the healing art. But surely the adoption of such a low standard of value implies great short-sightedness, and still greater ignorance of the history of science; as if the more general and abstract the truth, the greater were not, of necessity, the number of practical consequences it involves, and the greater, ultimately, the practical gain. Were it needful, a thousand instances might be brought from the annals of invention to illustrate this truth.

When knowledge, from being made subservient merely to the bodily wants and comforts of man, came to be cultivated for its own sake, he first became conscious of his true dignity and power. The springing up of this impulse in the various branches of human inquiry, was a great epoch in the history of all sciences, or rather might be said to constitute their first origin; for it was thus that art became science. It was then that inquiry into Nature grew to be a worthy and ennobling pursuit; and in reward for this obedience of man to his higher instincts, once a right method of inquiry found, the introduction of this spirit into the cultivation of each department of knowledge marked the commencement of a brilliant series of discoveries, which brought immense accessions to his power. In the study of the phenomena of life, the change was slow to make. Happily, however, for the advancement even of practical medicine itself, physiology is now extensively cultivated as a pure science. Numberless works of great merit are written upon it; chairs are instituted for the teaching of it; endowments are made in support of its votaries; and, in all civilised countries, numbers of eminent men dedicate their lives to its advancement. Thus in this branch of human knowledge, also, the division between the practical and the speculative, the *experimenta fructifera* and *experimenta lucifera*, is final and complete.

We must all rejoice, therefore, that this Association has at length recognised this great principle, in requiring separate reports on the subjects of physiology and practical medicine. Other motives, no less imperative, had indeed rendered such division necessary; for, so great is now the number of labourers in the field, so rapid the accumulation of new facts, and so active the development of new ideas, that the task of the mere annalist in either branch is one of

no small labour. In that of anatomy and physiology, indeed, it may be questioned how far still further division might not be of advantage; for the character of the investigations now made into the phenomena of life is so various and minute, and the record of them is made public in so many different languages, that to give a critical view of the whole—and such alone is of any great value—demands a combination of acquirements rarely to be met with in a single individual. For my own part, I have painfully felt, in many particulars, my own want of the necessary qualifications for the task. To be consistent, too, this, the pure science of our profession, should have been placed in the hands of a professed physiologist, and not in those of a practitioner; and this for many reasons.

To enter with confidence on a subject of such scope and difficulty; to fathom the various and profound questions which the record of even one year's progress raises for discussion, had required much sustained thought in calm and retirement. How utterly impossible it is to realise this, amid the incessant interruptions to which those engaged in the active duties of our profession are so peculiarly liable, is, practically, known to you all. Highly as I estimate the honour of being appointed to this office of being your annalist for the year in this most important branch of our studies, it would have been unpardonable in me to undertake it, had I not known that, perhaps, the majority of the members of this Association might plead these same obstructions; and many, probably, with still greater force. I trust, however, that they will not the less be admitted as a claim for your kind indulgence for the many imperfections of my work.

But, it is time to enter on my subject. Cuvier, in a fine passage in the *Régne Animal*, thus attempts to define it:—

"Life," he says, "is a vortex, more or less rapid, more or less complex, whose direction is constant, and which ever bears along molecules of the same kind; but in which individual molecules are ever entering in and passing out, in such sort, that the *form* of the living body is more essential to it than its *matter*."

This passage sums up, in lucid statement, the two fundamental conditions of animal life that lie open to observation;—to wit, chemical change, and a specific form. These two conditions, under the heads of animal chemistry, and structural anatomy, are now

being investigated, in all their manifestations, with an activity unparalleled in the history of our science, and under all the advantages which improved methods and more perfect instruments offer. It was not, perhaps, surprising that Cuvier should have attached the greater importance to the *form*,—to the anatomy than to the chemical change.

For although the discovery of the nature of respiration,—the complement of that of circulation, and the most important in the whole history of physiology,—had already been made, and might alone, and above all others, one would have thought, have awakened men's minds to the fundamental importance of chemical change in the phenomena of life, yet was this by no means its immediate result. Scarcely, at first, were the simplest and most obvious of its profound and manifold relations to these phenomena clearly perceived; so often have the most striking facts to be impressed on our minds before we come to feel their full force and significance. And even now that organic chemistry has grown to a vast body of science, and that relations of the most interesting kind have been unfolded by its means, there are few among us, I believe, who yet fully apprehend its true scope and importance. Some there are who still refuse its help in our science, and who put no faith in its interpretations. Such persons would do well to consider, for a moment, the great fact of respiration, and its incessant necessity to the maintenance of the higher functions of life. Why must the animal take in oxygen every moment or immediately die, or, at least, pass into a state of vegetative life, in which all the higher faculties are dormant? Why, again, when more motor power is expended, is there commensurate provision for increase of respiration, for more rapid chemical change? We have lost sight of the deep significance of these facts through our familiarity with them; and their true relations have been further obscured to us by our involuntary habit of contemplating this function in the character of a *want*, as known to us by our own consciousness, and as expressed in the struggles of the breathing animal, when the access of air is limited or prevented. Seldom in our habitual view of the act of breathing do our thoughts carry us beyond this. But, to the chemist, it ever appears, in ultimate analysis, simply as the provision for the introduction of oxygen into the economy, for the purposes of chemical change,—of combustion of so much matter.

That this combustion should be the source of animal heat, is simple enough; that it should bear a direct and essential relation to the creation of mechanical force, in such sort, that the latter may be—to use the word in its mathematical sense—a *function* of the former, is what, from the numerous analogies in nature, we may well conceive, though the intermediate mechanism be as yet concealed from us; but that this mere chemical change should be equally, and as incessantly essential to the manifestation of the functions of mind, is a fact which must ever remain a mystery to us. But what fact so strikingly asserts the claims of organic chemistry upon our attention, and the deep and intimate relations it bears to the most subtle phenomena of life. But if its claims on our attention be thus great, not less are those on our confidence, which its methods of investigation warrant. For all its problems are, in the last resort, resolved by the balance;—that severe and impartial instrument, whose introduction into chemical investigation by Lavoisier has changed the whole face of science, and which, appealing as it does to the most unalterable attribute of matter, deserves on that account a degree of confidence in its indications which few other methods of investigation justify.*

I have premised these few considerations, partly, because they appear to me, in some measure, called for by the present unsettled state of opinion on these subjects among professional men; but still more, because they put in a clear view the motives that have guided me in the division of my subject, and furnish my best justification for having given more space and detail to one branch of it than many might think required by its importance. This division, I need scarcely add, is that already suggested by the passage I have quoted from Cuvier, and has reference to the great movement of

* "It was not lightly," says Dumas, in his admirable *Lectures on the Philosophy of Chemistry*, "that Lavoisier made choice of this instrument. In this choice he was guided by a new and profound thought. For him all the phenomena of chemistry were due to displacements of matter, to the union and separation of different bodies. *Nothing is lost, nothing is created:* this was his motto, this his thought; and in the very first application of it he did away, for ever, with a great error." It was the absence of this thought which limited the discoveries of Priestley, brilliant as they were, in their influence on chemical philosophy. "Without doubt," says the same eloquent teacher, "Priestley was ill prepared to work a revolution in natural philosophy; but with his singular penetration, and with his felicitous art of taking advantage of every chance observation, he would probably have done so, had he only had the idea to endow himself with one instrument more."

matter through the living body, on the one hand, and to the organised forms and the actions of this matter in the sense of vital functions, on the other; in other words, it is the now familiar division into organic chemistry and structural anatomy, looking upon function as the result of their mutual relations. I shall begin with recording the progress of the former.

Although the past year has not been so prolific in new and comprehensive principles as the years immediately preceding, its contributions are by no means few nor unimportant. Numerous original investigations, of great value in particular departments, have been made public; and the novel views with which Liebig, Dumas, and other eminent chemists, have lately startled and so deeply interested the public mind, have received much interesting illustration and development, together, also, with some correction.

The first in rank and importance, in both points of view, stands the admirable series of papers on "Aliments and their Transformations," by Dumas and his fellow labourers.

Two of these had appeared in the previous year, in the *Records of the Academy of Sciences;* but their recent publication, in a separate volume, (the admirable *Memoires de Chimie* of Dumas,) and the circumstance of their not having been noticed in any of our former reports, give me the opportunity of passing them in review.

In the paper on the "Neutral Azotised Matters of Organised Bodies," by MM. Dumas and Cahours, the authors set out by stating that in an *Essay on Physiological Chemistry,* published eighteen months before, by the former and M. Boussingault,* it was laid down in principle,—that albumen, caseine, and fibrine, exist as such in plants; that they pass, ready formed, into the bodies of herbivora, and through these into those of the carnivora; but that plants alone have the power to fabricate these three products, which animals afterwards appropriate, whether to assimilate or consume, according to the needs of their life; and, further, in the same essay it was held, that these same principles might be extended to fatty bodies, which, according to this doctrine, originate wholly in plants, and come to play in animals the part of matter for combustion, or to be temporarily laid up in the tissues.

* *Chimie Statique des Etres Organisés.*

That, moreover, in the essay alluded to, they had recognised the necessity of grouping together all the organic bodies which have the property of passing into lactic acid by fermentation, such as sugar, starch, and their equivalents; bodies which enter as a staple and important element into the nutriment of man and animals, and are likewise produced by plants alone; views which, together with further developments, were summed up as a consistent whole in the following table:—

THE VEGETABLE	THE ANIMAL
Produces .. neutral azotised matters,	*Consumes* neutral azotised matters,
" fatty matters,	" fatty matters,
" sugar, starch, gums.	" sugar, starch, gums.
Decomposes carbonic acid,	*Produces* carbonic acid,
" water,	" water,
" ammoniacal salts.	" ammoniacal salts.
Extricates oxygen.	*Consumes* oxygen.
Absorbs .. heat,	*Produces* heat,
" electricity.	" electricity.
Is an apparatus of reduction.	Is an apparatus of oxidation.
Is motionless.	Is locomotive.

The authors add, that although these laws have reference to a certain number of facts and principles already known, they constitute as a whole, and in this, their correlation, a system which they have a right to consider as new.

As far as the neutral azotised matters are concerned, they remark, in the memoir of which I am now giving an abstract, that the indispensable obligation under which all animals are placed, of making these matters a part of their food, almost demonstrates of itself that they have no power to create them. But to place this result in its full evidence, it is necessary to follow these matters through the organism, and ascertain their final destination. Now, it is extremely easy to prove that they are essentially represented by the urea which in man and the herbivora forms the principal product of urine, and by the uric acid which in birds and reptiles takes the place of this principle.

Abstraction made of the excrements, an adult man absorbs each day a quantity of neutral azotised matters representing scarcely more than fifteen or sixteen "grammes" of nitrogen; a quantity which reappears in its whole amount in the 30.32 "grammes" of urea eliminated with the urine, in twenty-four hours.

It was with the object of verifying, controlling, and limiting whatever of truth these conclusions contain in relation to the neutral azotised matters, that the experiments which form the body of the memoir now under consideration, were undertaken.

My space does not allow me to do more than to give the general conclusions they serve to establish, and which are thus stated in the memoir itself:—

1. "Albumen possesses the same composition in all animals, and, therefore, *à fortiori*, in all the fluids of a single animal.

"Vegetable albumen differs in nowise from animal albumen, as to elementary composition, only it is not accompanied by free soda, (according to Liebig, bi-basic phosphate of soda,) the contrary being ordinarily the case with animal albumen.

2. "Caseine furnished by herbivorous animals is always of one composition, and as nearly as possible identical in properties.

"In woman, however, who by her habits of life approaches the carnivora, the milk furnishes a species of caseine, which, although similar in composition to that of the herbivora, nevertheless possesses properties of such kind, that it may be one day necessary to establish a distinction between these bodies.

"In the blood of the ox a substance is found which seems identical with caseine, both in elementary composition and in general properties.. The flour of the cerealia likewise contains a body which may be classed with caseine, on the same grounds.

"Moreover, the caseine of the milk of herbivora, that of the milk of women, the caseine of blood, and that of flour, possess exactly the same composition as albumen; so that these are certainly isomeric bodies."

Thus far it will be seen that M. Dumas' results correspond with those of the school of Giessen, with which Liebig's works have made us all so familiar, and which, collectively, constitute the great discovery of recent times in organic chemistry; but at this point there arises an important difference between them.

M. Dumas contends that fibrine is not, as Liebig and his followers assert, identical with albumen, inasmuch as, although the difference be slight, it indisputably contains more nitrogen and less carbon than the latter.

The following is the mean result of his analyses of this body, as found in the blood of man:—

Carbon	52.78
Hydrogen	6.96
Nitrogen	16.78
Oxygen	23.48

The recent investigations of Mulder, however, of which I shall have to speak further on, cast some doubt over these results, and explain the difference between them and those previously current, upon other grounds.

M. Dumas records, also, several analyses of proteine, which were made with the greatest care. The following is the formula which he regards as best representing the constitution of this body:—

Carbon	48	54.44
Hydrogen	37	6.99
Nitrogen	12	15.88
Oxygen	15	22.69
Total		100.00

In regard to the composition of legumine, also, which on account of its abundance, and the important part it evidently plays as nutriment, in the seeds of leguminous plants, demands particular attention, M. Dumas differs from Liebig. He denies that this body is, as the latter had asserted, identical with caseine, and states that it contains, without doubt, more nitrogen and less carbon. He is of opinion that it consists of a mixture or combination of albumen or caseine with some other body richer in nitrogen, which modifies its most important properties. As, however, on being dissolved in hydrochloric acid it communicates the same properties as albumen, it is easy to suppose that, under the influence of the gastric juice, this matter furnishes the same soluble products as albumen itself.

Besides these four principal products—albumen, caseine, fibrine, and legumine—the authors recognise two others—glutine and vitelline—so closely related to these by their mode of action with hydrochloric acid, as to blend with them in one group, however distinct their properties might at first sight appear.

This highly important memoir concludes with a theory of nutrition, which differs in many essential points from that generally received. Its peculiar features are these:—"That for the purposes of life, and especially for the maintenance of animal heat, it is

necessary that the great process of combustion should incessantly go on. That the materials for this combustion are furnished *directly* by the blood; and that, as they consume away, they must be restored to the blood either by the food or, failing that source, by matter from the living tissues, which thereby suffer damage. That the art of nutrition consists, therefore, in protecting the tissues from this damage, by restoring to the blood, at the same rate in which they waste away, the materials of which this fluid is itself composed."

Now, in protracted abstinence, there is no doubt that the tissues may suffer serious damage in this way. The general waste of the body in long illnesses, and also many forms of ulceration of the cornea and of cartilages, together with various destructions of parts resulting from habitually defective nutriment, are instances of this. It is equally certain that a large class of diseases, having no fixed place in our present systems of nosology, and now, through our ignorance of their true pathology, vaguely and variously classed in accordance with functional characters, or other principles still more general and undefined, will, eventually, be found to consist in abnormal destruction of tissue, by excessive action of oxygen, the great agent of change in the living economy, on parts whose vitality and power of resistance have been weakened by other causes. That this is the case with various forms of insanity and other obscure affections of the nervous centres, now referred to the vague heads of softening, atrophy, and other merely descriptive terms, admits already of complete demonstration. But, surely, M. Dumas cannot mean to maintain, that in the normal state of nutrition the tissues themselves suffer no change nor destruction. The doctrine generally received, and it seems to be one founded on the strictest induction, is, that in every functional act, a part of their substance necessarily, and by the very fact, loses its vitality and suffers chemical change, in effect of the great process of oxidation incessantly going on. According to this doctrine, therefore, the blood is not, as Dumas assumes, drained of its albuminous principles in furnishing, directly, material for combustion, but in repairing the losses which the tissues suffer, in prey to this process, and of which, under this view, they, and not the blood, are, as far as these principles are concerned, the great feeders. But the truth will, probably, eventually prove to

lie between these two doctrines; and, indeed, the researches of Mulder, to be presently mentioned, show that the proteine compounds of the blood are incessantly being oxidised in the act of respiration.

In the memoir before us M. Dumas follows these matters to their final destination, in the following terms:—

"As the nitrogen they contain is found again, entire, in the urine, under the form of urea, it only remains to inquire in what urea differs from the neutral azotised matters, from which it is derived. Now, the beautiful researches of M. Wöhler have taught us, that urea may be produced by a modification of cyanate of ammonia, which, itself, consists of an oxide of cyanogen and an oxide of ammonium. Thus, there pass out of the animal four oxides, namely, carbonic acid, water, cyanic acid, and oxide of ammonium, the two latter, in their modified combination, constituting urea; so that it is by a true process of combustion that the azotised matter of the food is converted into urea."

This deduction, which the admirable researches of Liebig* and Wöhler, on the transformations of uric acid, first enabled us to draw, and which is developed with great clearness by Liebig, in his work on *Organic Chemistry in its Applications to Physiology*, is one of vast practical importance. For it is well known, and especially in regard to urinary ingredients, though equally true of the other excretions, that the exact chemical form in which these excrementitious principles shall appear, is liable to wide variation, and that their extrication in the blood in forms difficult of elimination, or otherwise noxious, is the source of a class of diseases which, from the present habits of society, is necessarily one of the most numerous we are called

* Who is there that does not enter into the feelings of these eminent men, in reading the following fervid words, in which Liebig gives an account of their discoveries?—"For my own part," he says, "I confess that I felt my whole nervous system thrilling, as if pervaded by an electric current, when Wöhler and myself discovered that *uric acid* and all its products, by a simple supply of oxygen, became resolved into *carbonic acid* and *urea*, thus showing that there existed a connection between urea and uric acid, such as had never before been dreamed of in its infinite simplicity; when our calculation proved that *allantoin*, the nitrogenous constituent of the urine of the fœtus of the cow, contains the elements of uric acid and urea; and when we succeeded in producing allantoin, with all its properties, from uric acid. Though few words passed between us in these investigations, how often have I seen the eyes of my friend glisten with delight."

upon to treat. But, if it be true that these principles are products of oxidation, and the fact admits of no doubt, it is easy to determine, with sufficient exactness, the conditions on which these variations depend. Where the weight of the body remains nearly constant, they must essentially consist in the amount and quality of nutritive materials absorbed, on the one hand, and on the volume of oxygen inspired on the other—conditions which, in the shape of diet, air, and exercise, the practitioner has largely under his control The great efficacy of a judicious regulation of these is practically known to us all, and such regulation forms a large part of all rational medicine. But it may still further enhance the value of these means in our eyes, to form a clear idea of the direct and certain power they give us, in availing ourselves of the natural resources of the living economy, of cutting at the root of numerous grave disorders, by altering the very form of the material cause on which they depend. We look in vain for such a power in the action of drugs, which, therefore, provide no substitute for it.

These considerations naturally lead us to the researches of Mulder,* on the oxidation of proteine. The pith of these researches lies in a very small compass. This chemist finds that fibrine, on being boiled in water in the open air, gradually absorbs oxygen. The result of this is the formation of two new products, one of which is freely soluble in water, the other insoluble. These products differ from proteine, in the former containing three, and the latter two more equivalents of oxygen than this body. It was simply to express this relation, and without wishing to attach any other significance to the terms, that Mulder named these products, respectively, tri-oxy, and bi-oxy-proteine,—names, however, which seem very ill-chosen.

Bi-oxy-proteine on being further boiled, with exposure to air, becomes, in the end, wholly converted into tri-oxy-proteine by the absorption of another equivalent of oxygen.

By treating albumen in the same way as fibrine, the tri-oxy-proteine is formed at once, without passing through the intermediate stage.

Mulder also finds these products in minute quantity in normal

* *Journal für Praktische Chemie*; Erdmann and Marchand, Marz 18, 1844.

blood, but in much larger proportion in the buffy coat, and in recent false membranes. He even asserts that the buffy coat contains no true fibrine, but is entirely made up of a compound of these two bodies. Does it not seem from this extremely probable, that the colourless corpuscles which so abound in inflammatory blood, and about which Dr. Carpenter and Mr. Wharton Jones are now waging war, may be nothing more than this so-called bi-oxy-proteine.

According to Mulder, the substance which Bouchardat obtained from the buffy coat, and which that chemist mistook for gelatine, is, in fact, tri-oxy-proteine. In accordance with this view, it will be remembered that Bouchardat obtained this substance, in large proportion from the buffy coat, but only in very minute quantity from healthy blood. But Mulder has also ascertained, by ultimate analysis, that it is perfectly identical in composition with the tri-oxy-proteine, obtained by boiling fibrine in water in the open air.

From this series of facts the author considers himself entitled to draw the following conclusions:—

1. "That, on exposure to air, proteine suffers oxidation at the temperature of the living body.*

2. "That this action is constantly going on in the lungs, at the expense of the fibrine of the blood.

3. "That the proteine thus oxidised is the principal, if not the only bearer of oxygen to the rest of the economy; and in support of this he argues, that the hæmatosine of the blood, from its small mass, cannot possibly suffice to absorb the large volume of oxygen taken into the body.

4. "That inflamed blood contains a much larger amount of oxidised proteine than normal blood, and, in fact, that the normal and inflammatory conditions differ chiefly in this circumstance; from which the author infers, that every cause which accelerates the breathing and the circulation, tends to bring on an inflammatory condition; and hence the readiness with which inflammation is set up in various febrile disorders, &c."

This last statement, as to the existence of a larger quantity of

* Scheerer has shown, that at common temperatures moist fibrine absorbs oxygen largely.—*Annalen der Pharmacie*, Bd. xi., S. 13. See also his *Mikroscopische und Chemische Untersuchungen.*

oxidised proteine in inflammatory blood, accords well with the observations of Dr. Polli,* on the coagulation of blood, which may therefore be fitly introduced here. This physician finds that the presence of carbonic acid in blood always retards coagulation. The more carbonic acid the blood itself contains, the slower is the coagulation, and the greater the chance of a buffy coat being formed; and a buffy coat being formed, without froth and over a dark clot, is always a sign of the blood being surcharged with carbonic acid.†

Mulder further remarks, at the end of his paper, that the flesh we eat must become oxidised in cooking, and, therefore, that the chief part of the proteine compounds we take in must enter the body in a state of oxidation.

* *Gazetta Medica di Milano*, Aprile 15, 1843; and *Paget's Report on the Progress of Anatomy and Physiology* for 1842–43.

† Two other conditions co-operate largely in the result, namely, the increased proportion of fat in inflamed blood, and the tendency of the red corpuscles to adhere in rolls. The opinion that fat is one cause why the buffy crust swims on the blood, has lately been confirmed by Baumhauer, who found in the buffy coat from the blood of a horse:—

Fat, soluble in ether	3.276
Buffy coat, insoluble in ether	96.724

The following observations on the other point are taken from Mr. Paget's excellent "Report," *passim*:—

"*Buffy Coat.*—The exact mode of the formation of the buffy coat has been well illustrated by Mr. Wharton Jones, whose observations on the blood I can in nearly all points confirm. He ascribes it, as Nasse and others did, chiefly to the tendency of the blood corpuscles to arrange themselves rapidly in rolls, (like rolls of coins,) which form a wide-meshed network, (as seen in a single layer under the microscope,) or a kind of spongework when they are kept in mass. In the former case the liquor sanguinis coagulates within the meshes of the rows of the corpuscles, and hence the distinctly mottled aspect of the layer when coagulated, as well as when first drawn. In the latter case the spongework formed by the rows of corpuscles contracts and squeezes out the liquor sanguinis, and permits the greater specific gravity of the corpuscles to come into play, so that they sink quickly and the liquor sanguinis floats to the top and coagulates in a distinct layer. In both cases the pale corpuscles remain with the separated liquor sanguinis, and are imbedded in its white coagulum. The attraction for each other, by which the corpuscles tend to unite in rolls, is so remarkably increased in the state of the blood in which a buffy coat is formed, that the early or instantaneous occurrence of this arrangement of them, as seen by the microscope in a single drop of blood, affords all the evidence which could be derived from the formation of a buffy coat on a large quantity." And, further, quoting again from the same report:—"According to Mr. T. Wharton Jones, the congestion which succeeds to the temporary acceleration of the capillary circulation in an inflamed part,

These investigations are undoubtedly very interesting, but, on the whole, perhaps the author attaches too much importance to them. Without the evidence they afford, it must have been clear to every one who had ever thought on the subject, that the proteine compounds undergo ready oxidation at the temperature of the living body, from the waste which these compounds suffer when the animal is deprived of food. Still more plainly does this appear in the carnivora, animals that subsist on food identical with the chief constituents of their own body; which last must, therefore, be incessantly furnishing fuel for the combustion by which their animal heat is maintained. Still, most physiologists will find it difficult to admit, without further investigation, that the fibrine of the blood is the chief bearer of oxygen to the tissues.

The observation that oxidised proteine is present in much larger quantity in inflamed than in healthy blood, is of value, were it only for its use in calling the attention of pathologists to the chemical phenomena of inflammation. Seeing the vast amount of chemical change that goes on in inflamed parts, the great destruction of old matter, and the formation of new products chemically different, it is surprising that these phenomena have so long been overlooked in the investigation. As the disturbance of the normal conditions of nutrition, by the introduction of a chemical agent, is the most frequent cause of inflammation, and the first step in the process, so an enlightened pathology will eventually show, that chemical agency plays, throughout, a fundamental part in the changes of which this process consists.*

is due to the red blood corpuscles adhering together, (in the manner already described,) and to the walls of the vessels till stagnation occurs; and he has shown that the same arrest of the blood takes place when capillaries are touched with a solution of salt, or when a stream of carbonic acid is directed against those of the frog's lung. From these last facts he suggests, with much probability, that the stoppage of the circulation in the capillaries when certain salts are added to the blood, and that which takes place in asphyxia, depend on a similar adhesion of the corpuscles. With regard to asphyxia, his observations agree in their tendency with those of Dr. John Reid on the stagnation of the blood, independent of any apparent mechanical hindrance, when nitrogen is inhaled; and the action of carbonic acid in making the corpuscles cohere in rolls and assume the most favourable condition for the formation of a buffy coat, gives additional probability to the observations already quoted from Dr. Polli."

* Scheerer has recently made some very valuable investigations in this direction, and has given the results in his *Mikroscopische und Chemische Untersuchungen*,— a very valuable work.

From the great amount of heat evolved in the seat of inflammation, and other equally significant circumstances, it might already be inferred, that increased oxidation is an important element of the morbid changes. All this was very clearly and explicitly developed by Liebig, in his chapter entitled "Theory of Disease."* Mulder's observations on the buffy coat are chiefly of value in substantiating this view, by precise and authentic evidence.

To state, however, as Mulder does, that the formation of a larger proportion of oxidised proteine in blood constitutes the only difference between the inflammatory and the normal conditions, is, I need scarcely remark, a very one-sided and limited view of the subject.

The next paper that falls under our notice is one by MM. Dumas, Boussingault, and Payen, on "The Fattening of Cattle and the Formation of Milk." The investigation, of which it communicates the results, was undertaken with a view to determine the source of the fatty matters which animals store away in their bodies, or furnish in the shape of butter.

The authors give, at the outset, a very full and fair statement of the grounds of the commonly received opinion, that animals form a part of their fat, at least, out of other principles. These gentlemen were, however, early led to believe that animals have no such power, but that they find their fatty, as well as their azotized materials, ready made for their use in plants. I need not give in detail the various considerations which first inclined them to this opinion, in spite of the great difficulties that stand in its way, and the numerous analogies in favour of the opposite doctrine. These considerations were principally founded on M. Payen's discovery of the great abundance, and almost universal diffusion of oleaginous principles in plants; on the fact, that certain oils taken in the food reappear unchanged in the milk; and, lastly, on Magendie's observations on the formation of chyle. This physiologist, in observing the phenomena of digestion in dogs, soon found that this fluid varies greatly in constitution, in accordance with variations in the qualities of the food.

The chyle formed under the use of a vegetable diet rich in

* *Chemistry in its Applications to Physiology and Pathology.*

starch or in sugar, (principles out of which it is commonly supposed animals form a great part of their fat,) as well as that which results from the digestion of lean flesh, is very poor in globules. Such chyle is translucent, serous, and gives up to ether a very small amount of fatty matter.

These facts certainly furnish, as far as they go, a strong presumption in favour of the view taken by our authors. But they lay still greater stress on certain facts touching the conditions which affect the formation and nature of butter. "If one-half of a cow's allowance of hay be replaced by an equivalent of turnip cake, still rich in oil, the beast keeps up its condition, but the milk furnishes a more fluid butter, and this butter possesses, to an intolerable degree, the savour peculiar to the oil of turnip." "What," the authors ask, "can be opposed to this direct observation, and how not conclude from it, that the fatty matters of the food pass into the milk, often little or not at all altered, in order to form butter!"*

To determine between these rival opinions, as to the source of fat in animals, was a matter of no mean importance. The question involves a cardinal point in animal chemistry:—nothing less, in fact, than to ascertain in what degree animals also have

* I need not point out the great value of the practical application of these observations to the quality of woman's milk, which is, undoubtedly, liable to equal variation under the influence of similar conditions. The judicious regulation of these, by proper modifications in the food of the nurse, furnishes us with the only means we have in our power, (but, fortunately, they are direct and powerful,) of making changes in the natural food of the infant at the breast. All good practitioners are, more or less, in the habit of availing themselves of them; but there can be no doubt that many disorders of the sucking child are still treated by nauseous and irritating drugs, which should find their natural and appropriate remedy in the judicious regulation of the diet of the mother. To what an extent this may be brought to bear on the nature of the milk, will appear from the fact cited by the authors of this memoir—that the butter of the cows of a single locality may vary to such a point, according as they are fed on green forage, or on dry fodder, as to contain, as in the case of the butter of the Vosges, for instance, from 66 of margarine to 100 of oleine in summer, up to 186 of margarine to 100 of oleine in winter. In the first case the cows graze on the mountain side; in the second, they feed in the stable on dry fodder; for the rest, every one must have remarked the more solid state of butter in winter, and that quite independently of the lower temperature of the season. Another interesting observation to be found in this memoir is, that the milk of cows fed chiefly on carrots approaches very nearly in quality to that of women, a fact which, it appears, is taken advantage of in France, for the nourishment of infants deprived of their mothers' breast.

the power, by a process entirely different from the oxidation by which their principal chemical products are extricated, to form new principles out of the elements of their food; to what extent, in short, they share in that constructive power which reaches so high a point in the plant, and which modern chemistry has shown to be the great characteristic of it.

When we reflect, moreover, on the large proportion in which fat is found as a constituent of nervous matter; on its singular proneness to accumulate in some individuals, and the no less difficulty of its assimilation by others; and, lastly, on its vast importance as a store for the maintenance of animal heat in time of need; we shall gain some idea of the great interest which the ultimate decision of this question must have, both for the physiologist and the practitioner.*

It must be confessed that M. Dumas' solution of it has the merit of simplicity, and derives much plausibility from Magendie's observations on the chyle, which have already been quoted; insomuch that, the chief obstacle in the way of its general acceptance would, at first sight, seem to be the difficulty of supposing that the herbivora find in their food all the fat deposited in their tissues or furnished in their milk. The production of butter by the cow offered an opportunity of putting this to the test of experiment, and our authors, therefore, undertook to determine by exact analysis, the ratio of the fatty matter contained in the daily allowance of certain cows submitted to the investigation, to the amount of butter furnished in the milk of the same animals. The results fully confirmed their anticipations. The experiments on which they were founded, and which were conducted on a large scale and with great accuracy, form the basis of the memoir now under consideration. My space does not allow me to give more than their general results. These sufficiently establish the following conclusions, which I give in nearly the words of the memoir:—

* An important remark, in connection with the uses of fat, has reference to its insolubility in water. It is this quality which especially fits fatty matters for forming a magazine of combustible matter in herbivorous animals; for as these animals, like all others, have to produce in the day a certain amount of heat, they undoubtedly effect this at the expense of the soluble (saccharine and starchy) products their blood contains, before attacking the insoluble compounds, such as the neutral fatty matters, which the chyle pours into it without ceasing.

1. That hay contains more fatty matter than the milk it serves to form; and that the same is the case with the other kinds of food on which cows and asses are kept.

2. That oil-cake increases the production of butter, but sometimes renders it more fluid, and may also give to it the taste of oil of grains, when this article enters too largely into the daily ration.

3. That the very fattening quality of maize is a result of the large quantity of oil it contains.

4. That there exists the most complete analogy between the production of milk and the fattening of cattle, as graziers had already surmised.

5. That the grazing ox, however, appropriates for man's use less, both of fatty and azotized matter, than the milch cow; and that the latter, therefore, in an economical point of view, is greatly to be preferred, if the object be to convert a pasture into products useful to man.

6. That the potato, beet, and carrot fatten only when associated with other products containing fatty matters, as straw, to wit, or bran and oil-cake.

7. That, for equal weights, gluten mixed with starch, and flesh rich in fat, have, for the pig, a fattening power which differs in the ratio of 1 : 2.

In conclusion, the authors argue that these results fully bear out their doctrine, that animals derive their fatty matters, ready made, from plants; and that the old opinion, that they form these matters anew out of other principles, remains a gratuitous supposition without foundation in facts.*

It will at once be seen, however, that these results do not sufficiently warrant such a conclusion, and that they by no means definitively settle the great question touching the origin of fat in

* I regret that I have not space for the many important observations with which this memoir is interspersed. I cannot forbear, however, from quoting one which I am sure will interest those country practitioners who avail themselves of the delightful privilege their position gives them, of finding relief from the cares and anxieties of their profession in the more serene occupations of farming. The fact to which I allude is this, that it appears, as the result of unimpeachable experiments, that the milch cow appropriates, for man's profit, a quantity of alimentary matter, which may more than double that withdrawn by the grazing ox from the same pasture; whence it follows, as the authors well remark, that whatever may tend to place the milk market on a footing entitled to public confidence is, in the highest degree, worthy of an intelligent administration.

animals generally. In the first place, they do not prove *directly* that fat is never formed anew by animals, but merely that such a supposition is, in the case of certain species, unnecessary for the explanation of the facts; and although even this, taken in connection with Magendie's observations, gives some plausibility to the doctrine as far as the herbivora which inhabit the land are concerned, yet this is a very limited field of inquiry; and in order to establish the general proposition, other cases have to be considered. The inhabitants of the ocean, also, accumulate oil and fat in enormous masses. Do these, too, find these matters ready formed in their food? I need only instance the cetacea, to show the extent of the difficulty which this question offers; for although, as Dumas remarks in reply to Liebig, these are not herbivorous, but carnivorous animals, this is only shifting the difficulty, and not removing it. The question in this case can only be solved by an investigation of the same purport as that of which an analysis has just been given. For obvious reasons, the conditions of the problem can never here be determined with the same exactness, but approximate results of much interest might be obtained by ascertaining the amount of fatty matter which those marine plants contain which furnish food to fish.*

Other instances of great difficulty were not slow to occur to the minds of the authors themselves, to shake their faith in the conclusions they had drawn from their first investigations. One of a very special kind, but bearing closely on the general question, is the formation of wax by bees. Huber having ascertained that bees fed on sugar still continue to make wax, was naturally led to infer that these insects have the faculty of forming the latter at the expense of the former. Our authors felt, nevertheless, that this conclusion was not unimpeachable, since in Huber's experiment no account was taken of the fat pre-existing in the bodies of the bees, and which evidently might have been the source of the wax they continued to make. They therefore resolved to examine the question

* Probably, after all, as already surmised by M. Dumas, the solution of the difficulty will be found in the remarkable property, first discovered by Count Rumford, and since more accurately investigated by M. Morren, and more lately still by Wöhler and Pfankuch, which certain infusoria possess of decomposing carbonic acid, after the manner of plants under the influence of light; a property which many considerations render probable will eventually be found of the greatest importance in the economy of the ocean.

anew, under a method of investigation in which this, and all the other conditions of the problem, should be accurately determined by the balance. The inquiry was entrusted to MM. Dumas and Milne Edwards, so pre-eminently qualified by their respective pursuits to join in the task.

The method of investigation adopted was the following:—A given number of bees were secluded in a glass hive and fed exclusively on honey. Before the experiment was begun, a large number of working bees from the same hive were analysed, in order to ascertain the amount of fat the body of each contained. The honey on which they were fed was accurately weighed, and its content in wax exactly determined. Finally, at the end of the experiment, which lasted from the 7th of July to the 8th of August, a certain number of the bees were again analysed for their fat, and the wax produced was accurately weighed. The results obtained are expressed in the following figures:—

	Grammes.
The fatty matter pre-existing in the body of each bee was estimated at ..	0.0018
That furnished to each bee in the course of the experiment did not exceed ..	0.0004
Fatty matter, which could be ultimately traced to aliments, did not amount, therefore, in each bee, to	0.0022
Whereas, in the course of the experiment, each produced in wax	0.0064
And after having furnished this abundant secretion, each contained in its body, whether in wax or in common fat	0.0042
Making a total of	0.0106

So that the wax produced *alone* nearly tripled the amount of fatty matter pre-existing in the bodies of the bees, and that contained in their food, together; thus showing that a vast proportion of this wax must have been formed at the expense of the saccharine matter of the honey, and confirming in the most complete way Huber's original and beautiful observation.

This admirable investigation is deeply interesting in many points of view, but in none more, perhaps, than in the beautiful evidence it gives of the competence of chemistry, in all that concerns the movement of matter through the living economy, to deal with the highest questions of physiology, and in the exemplification it affords of the unimpeachable authority the balance gives to observations in this science.

In the discussion that ensued on the reading of this memoir, M. Milne Edwards argued that the results of this inquiry in no wise invalidate the inferences drawn from the former investigation on the fattening of animals, since the wax, the product here treated of, is a *secretion*, and is formed, therefore, under quite different physiological conditions. But the chemical fact does not the less remain the same; to wit, that in the bodies of animals fatty principles may be formed by a tranformation of sugar. The claims of wax to be considered as one of this family of products, have been completely established by the researches of M. Lewy,* proving the ready conversion of this substance into stearic and margaric acids; and, again, by those of M. Gerhard, which show that it furnishes, with nitric acid, exactly the same products as other fatty matters. That such transformation of sugar is not a peculiarity of bees, but is shared with them by vegetable feeders generally, so that these likewise form fat out of the saccharine elements of their food, is rendered all but certain by other considerations. A very interesting fact, showing the facility with which this chemical change takes place, has recently been discovered by MM. Pelouze and Gelis. These chemists found that, "If, instead of yeast, a small quantity of white cheese-curd be added to a solution of common sugar, and kept at a temperature of 76° to 80°, and, moreover, some chalk be added to maintain the fluid in a neutral state, a lively evolution of gas takes place; the sugar totally disappears, carbonic acid and hydrogen are given off in the gaseous state, and we find in the fluid a copious amount of butyric acid—one of the most interesting of the organic acids, which until recently was known only as a constituent of milk and butter."†

In connection with the physiological bearing of this experiment, MM. Pelouze and Gelis think it important to remark, "that this change of sugar into butyric acid takes place without any considerable increase of temperature, and without the presence of those energetic substances which could destroy the equilibrium, or affect the vitality of animal tissues, but that this transformation is

* *Journal für Praktische Chemie:* Erdmann and Marchand. See for another paper, by the same author, on the "Analysis of a Particular Wax of Vegetable Origin," No. 1, 1844, of the same Journal.

† *Lancet*, May 12, 1844.

effected under very simple conditions, and in substances employed by nature herself."

With evidence of such various kind, thus pressing upon us from all quarters, and all tending to one point, it becomes impossible to resist the conclusion that animals have the faculty of forming fat out of the saccharine elements of their food, as Liebig has all along held. It is no small testimony to the sagacity of this distinguished chemist, that the first complete and rigorous demonstration of the fact, (in the instance of bees,) should have been furnished by the very men who had done so much to render it probable that animals have no such power. Still, the original paper by Dumas and Boussingault on the fattening of cattle, and formation of milk, will ever be highly interesting from its great intrinsic value; and, for this reason, I have given an abstract of it a place in this Report, although the chief conclusion it was intended to establish is now set aside.

For the rest, all analogy concurs to show, as M. Dumas has so well remarked, that the conversion of sugar into fat in the living body most probably occurs in the *primæ viæ*, by a special fermentation, and not in the blood. The important remark, that the saccharine element of the milk of the *carnivora* must be formed by a process, which is probably exactly the reverse of this, namely, the conversion of fat into sugar of milk, appears to have entirely escaped the attention of physiologists. This product cannot be derived as such from the food of these animals, which contains no element of the kind, while many analogies render it probable that the fat of their bodies is its true source.* The subject is well worthy of investigation from its close bearing on the pathology of that most interesting and obscure disease, diabetes.

From these investigations into the nature and source of the constituents of animal bodies, furnished to them in their food, I pass on to the process of digestion by which they are prepared for absorption into the blood. During the past year many valuable contributions have been made to our knowledge of this function. The foremost in rank and importance is an elaborate work by M. Blondlot, entitled *Traité Analytique de la Digestion.* The method of investigation adopted by the author is one which, as the gift of

* Dr. Prout would probably say "gelatine."

accident, has already been fruitful of valuable results. Taking the hint from the now celebrated case of St. Martin, M. Blondlot conceived the idea of forming, by operation, fistulous openings in the stomachs of dogs. After many fruitless attempts he completely succeeded in two young dogs, and made the opening permanent in each, by fixing a silver canula in it. One of these dogs is still alive and in his possession, and although for two years past M. Blondlot has continued to make use of the fistula for procuring gastric juice and chyme, and for introducing thermometers, tubes, and sounds into the stomach, the dog does not the less remain in perfect health. Not only has it completed its growth, but it has grown fat, lively and alert, and enjoys an excellent appetite.

With these opportunities, alike favourable for observation and experiment, M. Blondlot continued his researches on the phenomena of digestion, for a long period. It will greatly simplify my task of giving an abstract of them, if I state at once that one of the chief conclusions to which they have led him is, that neither the saliva nor the bile take any essential part, whether in the process of digestion or in that of chylification.

The chief office of the saliva is, he asserts, mechanical, and intended merely to facilitate the acts of mastication and swallowing. He has found, by repeated experiment, that in the digestion of whatever kind of food, in gastric juice out of the body, the addition of saliva never accelerates the process in the slightest degree. These statements are in entire accordance with the observations of Berzelius and Dr. Beaumont; and although so widely at variance with the more recent and elaborate researches of Dr. Wright, there are many and weighty reasons for believing that they come very near the truth. At least, they derive very powerful support from comparative anatomy; for, in the gradual simplification of the digestive apparatus in the scale of animals, the salivary glands are the first to disappear, and that, too, in animals in which the digestive function is singularly active. I cannot but think that this fact alone offers a strong reason for believing that the last-named physiologist has erred in attributing to saliva such a very essential part in digestion. M. Blondlot further denies Liebig's assertion, that the chief office of this fluid is, (in virtue of its tendency to inclose air in the shape of froth,) to introduce air into the stomach; "for," he remarks, "if

an animal be suddenly killed during digestion, and the stomach examined, its walls will be found closely applied to the alimentary mass, in which it is impossible to discover any gaseous mixture." M. Blondlot has been unable to detect sulpho-cyanogen in saliva, and is of opinion that the substance named ptyaline* is not peculiar to saliva, but common to all mucous fluids.

In considering the uses of the bile, he discusses, one after another, the principal hypotheses which have been made touching the part it plays in chylification. He deals, first, with the office ascribed to it of neutralising the acid of the chyme, and inquires how the very small quantity of alkali this fluid contains can suffice for the purpose, since M. Thenard has proved that a few drops of vinegar are enough to neutralise all the bile found in the gall-bladder of an ox. He adds that the matters poured from the stomach into the duodenum retain their acidity after their mixture with the bile, to nearly the same degree as before, and only lose it as they travel along the intestine; and finally remarks, that if the bile were meant to neutralise the acid of the gastric juice, how should it happen that in some animals it is poured directly into the stomach, in such manner as must, under this supposition, destroy the chemical properties of the special fluid this viscus secretes, before chymification is completed. That the alkali of the bile assists in dissolving the fatty matters of the food, he considers as still less tenable, since what little this fluid contains must be already neutralised by the acid of the chyme. Nor does the author concede any office of the kind to the biliary matter itself, which has been thought to render fat more fit for absorption by forming an emulsion with it. To establish these opinions on sure grounds, he placed ligatures on the common biliary duct in several dogs. The intestines being now cleared out by a dose of croton oil, various aliments, such as bread and milk, flesh, and cheese, were eaten by the dog in each experiment with some eagerness. At the end of five or six hours, the animal being killed and immediately opened, M. Blondlot invariably found, in direct opposition to the observations of Brodie, and Tiedemann and Gmelin, a white and

* These two last statements throw some little discredit over the chemical part of M. Blondlot's investigations. Saliva certainly does contain sulpho-cyanogen; and the fact that this fluid has the power of converting starch into sugar, while other mucous fluids have not, is strong presumptive evidence in favour of ptyaline being, to some extent, a peculiar principle.—See Dr. Wright's papers on "Saliva," in the *Lancet, passim.*

well-elaborated chyle in the thoracic duct and in the lacteals of the mesentery.

All these questions are now in the way of being finally settled by the very ingenious experiments of Schwann, of which I shall have to give an account presently.

The pancreatic fluid, M. Blondlot is inclined to regard as chiefly intended to lessen the acrimony of the bile by dilution; a view which, I need scarcely remark, is extremely unphilosophical, and entirely without sanction in anything we know of the animal economy.

The saliva, bile, and pancreatic juice being thus disposed of, the investigation reduced itself in the mind of the author, to an exact determination of the effect of the gastric juice on the various aliments on which animals feed.

A great number of interesting experiments were made with this object, for the detail of which the reader is referred to the work itself. It will be sufficient here to state, that their general result was to lead M. Blondlot to divide the matters of which aliments are made up, into three separate categories, according to the nature of the modification they severally undergo.

The first comprises those substances which quit the stomach in the same form in which they entered it: such is the case with mucus, whether solid or liquid; with resins, woody fibre, and fecula. According to the author, not only do these matters traverse the stomach unaltered, but the whole course of the intestines also; and without giving to the system any nutritive principle. It is important to understand that by the word "fecula" is here meant starch in its raw state, and as it exists in the form of minute globules, now well known as objects of singular beauty when viewed through the microscope by polarised light. These globules may be discovered in great abundance in the *excrements* of many herbivora that eat largely of amylaceous food. The ruminants, however, and graminivorous birds seem to have the power of crushing them, and the true starch thus becoming liberated in a soluble state—a change effected under other circumstances by heat*—is readily absorbed.

* This fact renders it probable that great advantage would accrue from scalding the oats and other amylaceous parts of the food of horses that admit of such treatment. The globules would thus be made to burst, and the starch, consequently, would be liberated in a soluble form, fit for absorption.

In this condition it passes into M. Blondlot's second category, which includes all substances that dissolve in gastric juice, exactly as in pure water. To this group belong liquid albumen, pectine, sugar, gum, and starch, in the form just spoken of; so that, for these substances, chymification is in reality nothing more than simple solution, in the utmost rigour of the term.

This statement is, however, in several important points, at variance with the opinions of many eminent chemists.

Thus, M. Dumas, reasoning probably from the extreme readiness with which sugar passes into lactic acid under the influence of animal membranes, and from the inevitable conversion of this acid, in common with organic acids in general, into carbonic acid by the action of respiration, should it find admission into the blood, was led to believe that all saccharine aliments undergo lactic fermentation in the stomach,* in order to become better fitted for the purposes of combustion. One does not see, however, what should prevent sugar from sharing what would appear to be the necessary fate of all soluble matters, in being absorbed without change; and it will be seen from the researches of Dr. Percy, that under certain conditions, and perhaps generally, it does enter the blood unaltered.

As far as the production of lactic acid is concerned, M. Blondlot's statements are decided and unequivocal:—"I can affirm," he says, "that having frequently analysed the liquid pressed out of aliments that had been some time in the stomach, I have never found in it *the slightest trace* of lactic acid;" a statement which derives additional weight from its entire accordance with the experience of Dr. Prout on the same point.

In regard to the question of the conversion of starch into sugar during digestion, however, these two observers are in direct opposition. Dr. Prout holds that the *reduction*, as he terms it, of all the forms of the saccharine principle, (meaning starch and its equivalents,) is accompanied by the development of a *low* sugar; and the great frequency of such a transformation, in a variety of circumstances, under the influence of agencies nearly related in their character to those which play an important part in digestion, is a strong *a priori* argument in favour of the view. M. Dumas also was, until recently, of the same opinion.

* This notion has since met its final overthrow at the hands of Professor Liebig.

M. Blondlot, on the other hand, denies the fact in the most unequivocal terms; and after remarking that MM. Sandras and Bouchardat have positively ascertained, by means of M. Biot's delicate instrument, that sugar is never formed in the stomach in the healthy state, he adds for himself, that he has sought, in vain, for this principle in starch that had been submitted for many hours to the action of gastric juice, whether within the stomach or at an artificial temperature without. The author, however, actually records but one experiment of the kind, and that one cannot be considered decisive, as it appears to have been made on fecula, or raw starch, a form in which, as we have already seen, the true starch is, in a great measure, protected from change by the capsule in which it is inclosed. This is not the state in which it is generally present in the stomach of man and animals; for here it is, for the most part, found liberated in a soluble form, whether by trituration or previous cooking.

I do not know whether or not the same objection applies to the investigations of MM. Sandras and Bouchardat, but it is much to be regretted that M. Blondlot did not avail himself of his unlimited opportunities to make experiments on a larger scale, and on starch in a state of solution. This is a question which it is highly desirable to have finally settled, since, besides involving a cardinal point in the theory of digestion, it derives additional and peculiar interest from its intimate connection with the pathology of diabetes.

I now pass on to M. Blondlot's third and last category. This comprises those substances, on which the solvent action of the gastric juice is as nothing, or nearly so, but which, under its influence, lose their cohesion, and are broken down into a molecular pulp. On some of these this effect appears to be produced merely by the *acid* of the gastric juice, and has nothing special in it. Such are the fleshy parts of fruits and succulent roots. For the others the change is of a special kind, and consists, in a peculiar disintegration of the aliment, by the gastric juice acting in its specific capacity. Among these substances he numbers fibrine, animal and vegetable albumen in the concrete state, caseine hardened by heat, matters that furnish gelatine, and, finally, gelatine itself—in short, putting out the last, the great group of proteine compounds, out of which the blood and tissues are made.

The most startling part of this statement is, that, according to the author, and in direct contradiction to the inference to which MM. Sandras and Bouchardat have been led by their recent researches, as well as to the opinion of chemists in general, these substances are not *dissolved* by the gastric juice, but merely converted into a pulp of extreme fineness. On this head his statements are so clear and explicit that I will venture to give them in nearly his own words.

"Another point," he says, "which it is important to establish well is, that the gastric juice does not really dissolve any of these substances, or at least that the solvent action it exerts upon them is all but insignificant. In fact I have ascertained, by numerous experiments, that fibrine, coagulated albumen and caseine, hardened by heat, are not dissolved by gastric juice in greater quantity than in water acidulated to the same degree by phosphoric or any other acid; and we all know that, diluted to this point, acids, of whatever kind, scarcely attack these matters at all. We have seen also that, after prolonged contact with these matters, at a temperature of 40° *centigr.*, the gastric juice gives with nitric acid and with bi-chloride of mercury, a precipitate so scanty in comparison with what remains undissolved, as to be not worthy of any consideration. When its action is complete the larger undissolved portion appears broken up into particles of extreme minuteness, which occupy the bottom of the vessel, and which, when shaken up, give to the liquid the look of an emulsion. If, in this state, a drop be put on a slip of glass and examined under the microscope, an immense number of irregular particles of various sizes are seen. The smallest are scarcely as large as blood-globules, the largest much larger than these." M. Blondlot elsewhere remarks that the whole has the closest resemblance to what Mr. Gulliver has named the *molecular base* of the chyle, as, in fact, these are, according to the author's view, identical matters.

This doctrine, that the proteine compounds are not dissolved in digestion, is however one that we must be cautious of adopting, and the more so as, according to M. Bouchardat's observations, even dilute muriatic acid, of the same degree of acidity as gastric juice, does, after a time, completely dissolve muscular fibre.

Touching the absorption of alimentary matters, M. Blondlot is of opinion that the undissolved matters, which, according to him,

comprise fat and proteine compounds, enter the system through the lacteals, while those held in solution, such as sugar and its equivalents, are taken up by the veins. As far as the general doctrine is concerned, he is probably right in this, since these are, undoubtedly, the channels through which the dissolved and the undissolved products of digestion severally enter the economy.

But the most startling statement in M. Blondlot's book is the assertion, in direct contradiction to Prout, Liebig, Gmelin, and a host of other eminent observers, that the gastric juice contains no muriatic acid. The experiments on which this assertion is founded, if exactly performed and rightly interpreted, would certainly be conclusive as to the fact; but there, surely, must be some fallacy here. It is difficult to admit on the faith of the observations of a single individual, however precise these observations may seem to be, that so many eminent chemists have been mistaken as to a fact so easy of determination. If it were really true that the gastric juice contains no muriatic acid, what a tissue of fine theories on the uses of common salt in the economy—giving, by decomposition, muriatic acid to the stomach, and soda to the bile, &c.—would at once vanish into air.

According to M. Blondlot, the true acid of the gastric juice is phosphoric acid; not free, however, but in combination, as an acid phosphate of lime. His inquiries as to the precise part which the different constituents of the gastric juice severally play in the digestive process, offer nothing sufficiently novel to require quotation or comment. As far as they go they confirm, in all fundamental points, the results arrived at by other physiologists. The following observations on agents that promote the secretion of gastric juice are of more interest, and may not be without practical value.

After speaking of the effect of mechanical irritation, he says:—"Chemical agents applied to the surface of the stomach, when its internal tunic has become turgid under the influence of food, have still more marked effects. Thus it has often happened to me, to render the flow of gastric juice much more rapid and abundant, in rolling the morsels of flesh I gave to my dogs in powdered pepper, in *sugar*, salt, magnesia, (usta,) carbonate of potash, &c. Daily observation shows that these substances favour digestion, and everything inclines us to believe that they do so in increasing the secretion of gastric juice, just as they do that of the saliva, pan-

creatic juice, and bile, when placed in relation with the excreting ducts of the glands which furnish these fluids."

Another French physiologist, M. Cl. Bernard, has since taken up the subject of digestion in a different way. His first object was to determine the exact amount and nature of the influence which the gastric juice has in the process. With this view he adopted the singular plan of injecting various aliments into the blood, now alone, and now mixed with gastric juice, and watching the difference in the result.* It is obvious however, that experiments made in such violation of all the conditions of nature, cannot lead to any profitable conclusion. In evidence of that indifference to the labours of the physiologists of other countries, which has become characteristic of his countrymen, M. Bernard shows his complete ignorance of many important investigations bearing on his subject, which have lately been undertaken in England and Germany. Had he been acquainted with the interesting researches of my friend, Dr. Percy, he would have seen at once that the elimination of sugar through the kidney, when this substance is injected into the blood, by no means warrants the conclusion he has attempted to draw from it.

Of Dr. Percy's researches, to which I now pass, the following are the principal results:—

1. When grape sugar is present in the blood in a certain quantity, a portion of it is speedily eliminated by the kidneys, and may be found in the urine.

2. When grape sugar is present in the blood only in small quantity, it does not pass into the urine in an appreciable degree. In this case it probably undergoes oxidation in the lungs.

3. When cane sugar is present in the blood in certain quantity, a portion of it passes into the urine as *cane* sugar; it does not appear to be converted, in the smallest proportion, into *grape* sugar during its passage through the blood.

4. When cane sugar is present in the blood in large quantity, it exerts a powerfully diuretic action, and the urine evacuated appears to be principally a solution of sugar.

5. When grape sugar is introduced into the stomach under conditions favourable to absorption, a portion of it is rapidly absorbed and passes into the urine.

* *Archives Générales de Médecine*, Mars, 1844.

6. When a dog is fed upon cane sugar and water, a portion of the sugar may be found in the urine.*

I need not point out the various interesting applications of these results to the pathology of diabetes, for whose especial elucidation they were undertaken.

Studying the phenomena of digestion from another point of view, MM. Gruby and Delafond appear to have made the curious discovery, that infusoria are developed in great numbers in the stomach and intestines of various animals. Four species are found in ruminants, seven in the horse, two in the dog, but only one in the pig. In the sheep they so abound, that the authors estimate their weight at one-fifth of that of the liquid in which they swim: they exist in enormous numbers in the two first stomachs, but in the third and fourth their skeletons only are found. In the intestinal canal of the horse, however, they are found alive in the narrow colon, and even as low as the rectum. From these circumstances, M. Gruby is led to infer that they perform an important office in the digestion of ruminants and other herbivorous animals, in converting their vegetable food into animal products, so as to render it more fit for their general nutrition.

Chemically speaking, however, there is no object to be gained in this; and it is, moreover, only throwing the difficulty on the infusoria, if difficulty there be. At all events, it cannot be admitted that the agency of these minute creatures has any essential physiological relation to the function of digestion, as M. Gruby would seem to believe.

To complete my account of this particular province of physiology, it will be necessary to advert to some recent investigations into the composition and uses of bile. A monograph on the subject has lately been published by M. Bouisson, which, although it does not contain much original matter, gives a very good epitome of our present knowledge upon it. The author begins with an account of the microscopic appearances of bile.

Healthy bile, if not concentrated, merely gives a yellow stain to the glass on which it is laid, and usually offers no definite objects. Sometimes there may be seen epithelial scales from the

* *Medical Gazette*, vol. xxxii., p. 124.

mucous membrane of the gall-bladder and duct, small crystals of cholesterine, and, if the bile be concentrated, irregular solid particles of biliary matter.

M. Bouisson next gives an historical account of the opinions that have prevailed at different times as to the composition of bile. He seems to have made no researches of his own on the point; and the conclusion he draws from a comparative study of the investigations of others, is, that the essential ingredient is a watery solution of a peculiar soap.*

He ascribes great influence to the bile in digestion, and holds that, as it travels along the bowel, by far the greater part of it is again taken into the system. He also considers that it promotes the formation and absorption of chyle in these several ways:—

1. By stimulating the intestine, and by preventing the decomposition of chyme.

2. By dissolving fatty matters, and thus favouring their absorption.

3. By taking part in the conversion of aliments, and especially in the formation of albumen out of other matters.†

4. By neutralising the acid of the chyme.

In his chapter on the differences of bile, according to age, sex, &c., the only part worth particular notice is that on the bile of the fœtus, which, according to the author, differs from the bile of the adult chiefly in the smaller proportion of choleate of soda, and corresponding predominance of colouring matter. The remaining portion of the work is chiefly pathological.

From this brief sketch it will be seen how widely M. Bouisson's opinions are at variance with those of M. Blondlot, as to the part which bile plays in chylification. There can be no doubt that the former physiologist greatly overrates its influence in this process. It may be remarked, in particular, that the recent investigations of chemists leave no foundation whatever for the opinion which M. Bouisson has adopted from Prout, that bile helps to form albumen out of other matters.

All these questions seem now to be in a fair way of being cleared up by the ingenious experiments of Dr. Th. Schwann, of

* See "Demarçay's Papers."

† Prout.

Löwen.* These experiments, which were made on dogs—those martyrs to experimental physiology—were performed in the following manner:—the common biliary duct was first tied, as in the investigations of Brodie, Blondlot, and others; but in order to prevent the poisonous effects which result from the detention of bile in the system, an artificial fistula was formed between the gall-bladder and walls of the belly, through which this fluid was allowed to escape. The experiment did not always succeed: many dogs died from the first effects of the operation, from peritonitis, and other causes. Some, however, suffered very little from it: in the course of a few days the wounds were perfectly healed, the artificial fistula was permanently established, and the animals had completely recovered from the effects of the operation, considered as such. The effects which followed were simply those which result from the absence of bile in the intestine, and from the waste of this fluid; effects which the professor had thus an opportunity of observing free from all complication.

The only object proposed in this, his first paper, was to determine how far the discharge of bile into the alimentary canal is necessary to life. It is clear, however, that these experiments cannot give a precise answer to this question, for the effects which followed were probably due quite as much, if not more, to the *waste* of bile as to its being shut off from the intestine. However this may be, the result was, that all the dogs experimented on gradually wasted away and died; and, what is very remarkable, died quite as soon as dogs starved to death by total privation of food. It is important however to notice, that for the first three or four days, many of them did not lose a single grain in weight; a result which cannot well be explained, unless by supposing that they ate more than before. But in the greater number there was very evident wasting as early as the third day, and this went on gradually up to their death, which was evidently the result of inanition. In two cases, however, there was a middle period in which the process of emaciation was for a time stayed. Thus, in the seventeenth experiment, after the first three days the dog gradually lost weight for three weeks, when it again began to gain, and continued to do so until it had nearly recovered its original weight; then followed another

* Versuche, um zu auszumitteln ob die Galle im Organismus eine für das Leben wesentliche Rolle spielt; *Müller's Archiv.*, heft ii., 1844.

period of wasting, which went on steadily until the dog died. In the fifth experiment similar alternations occurred. The author is of opinion that the only probable explanation of these anomalies is, that in the intermediate period the continuity of the duct was temporarily restored.

In some of the experiments the dogs were allowed to lick up the bile as it escaped on the floor, which they did very eagerly, but they did not the less continue to lose weight exactly as before.

The final result in all these experiments furnishes conclusive proof, if indeed proof were needed, that the bile is not a merely excrementitious fluid, but that, after its discharge into the intestines, it is destined to serve an ulterior purpose of essential importance to life. The measure of this is well given by the striking fact, which few, perhaps, would have anticipated, that when this fluid is allowed to run to waste in dogs, they die of inanition quite as soon as if starved to death by total privation of food. For the precise interpretation of this remarkable result we must await that section of Professor Schwann's researches, in which the part which bile performs in digestion and chylification is to be fully considered. Meanwhile, the observations made in his eighteenth experiment will be found of some interest in their bearing on this point. When the dog which was the subject of them was already at the point of death, Professor Schwann killed it by blood-letting, in order to procure its blood for analysis. He then made a careful examination of the body, and ascertained the following facts:—

"In the abdomen there were no traces of actual inflammation, and only a few partial adhesions. The gall-duct was so completely obliterated, that the hepatic duct passed by a continuous curve into the cystic duct, without showing any trace of the former insertion of the ductus choledochus communis. There was still some fat remaining about the mesentery.

"The lacteals of the mesentery were translucent and almost empty. The thoracic duct, within the chest, contained a moderate quantity of lymph of whitish colour, like milk diluted with water. This lymph coagulated in from ten to fifteen minutes, and, therefore, contained fibrine. Under the microscope there were found in it, besides lymph-corpuscles, a great number of fat-globules of all sizes, like those of milk, but some much larger than those usually found in that fluid. The stomach was filled

with curdled milk—(the animal had been fed on milk the twenty-four hours preceding death.) The upper half of the small intestine, also, contained a milk-like substance, which was very fluid in the duodenum, but in the jejunum rather more consistent. The whole upper half of the intestine showed no trace of yellow colour."

These results certainly give some colour to M. Blondlot's opinion, that bile is of small importance in digestion; for the fluid here described as taken from the thoracic duct, had all the characteristics of well-elaborated chyle. But, however this may be, Professor Schwann's investigation, taken as a whole, gives strong support to Liebig's theory,—that the greater part of the bile is re-absorbed, and that its principal importance is as fuel for respiration. For numerous and well-authenticated cases have taught us, that the common gall-duct may be permanently obliterated in man, and yet life continue for many years; so that it seems fair to conclude, that the speedy death which followed Schwann's experiments on dogs, resulted not so much from detriment to digestion, as from the waste of bile as material for the use of the living economy. And since the bile which is re-absorbed never re-appears in any of the other secretions, its carbon and hydrogen must evidently pass off in combination with oxygen, as carbonic acid and water. That a great part of the bile is re-absorbed in the higher animals, might almost be inferred, without further investigation, from the enormous size of their livers; for the amount of solid matter secreted by glands, bears some general proportion to their size, and it surely cannot be supposed that the very small quantity of biliary matter voided daily, can represent more than a small proportion of the secretion of such a vast organ. I have no doubt that when we shall have succeeded in ascertaining the exact amount of bile secreted daily, it will be found far to exceed all our present estimates of it.

In the same number of *Müller's Archiv* which contains Schwann's investigations, is a communication by Dr. Platner,* of Heidelberg, stating, that he has succeeded in obtaining the electro-negative body, which is supposed to be the essential constituent of bile, in a state of crystallisation, both pure and in combination with

* "Krystallisation der Gallensäure und des gallensauren Natrons," beobachtet von Dr. E. A. Platner, Privatdocent in Heidelberg.

soda, as it is supposed to exist in bile itself. This, if confirmed, would be, I need scarcely remark, a result of considerable importance, since it would authenticate the views of Liebig, Demarçay, and Kemp, on the constitution of bile, by exactly that kind of evidence of which they now stand in need; and would for ever do away with the old notions as to the very composite character of this secretion.

Dr. Platner gives in detail the processes by which he obtains these products. Before, however, his results can be finally admitted, it will be necessary for him to prove, by ultimate analysis, the identity of his crystalline body with the electro-negative body of M. Demarçay. This he has not hitherto done.

Having now gone through the history of the *raw material* by which animal life is fed, as also that of the process by which it is prepared for use, I pass at once to consider the dead chemical forms in which it is again thrown off, after having played its part in the living economy.

Two principal organs, the lungs and the kidney, are charged with throwing off the effete material: by the former it is cast off in the shape of carbonic acid and water; by the latter in that of urea and other oxidised products.

Within the past year Scharling* has published an extensive series of researches on the elimination of carbonic acid. He found, as might have been anticipated, that the amount evolved in a given time is liable to wide variations in the same individual; that it increases rapidly immediately after taking food, so as to reach its highest point soon after the principal meal of the day; that it sinks very low in the state of hunger; but that in sleep it reaches a lower point still. In one case, for example, the quantities evolved in the same space of time,—first, about an hour and a half after dinner; next, when the individual was very hungry, immediately before breakfast; and, lastly, when sleeping,—were represented by the figures 165, 130, 100. These results are scarcely worth the pains they cost, since our present precise knowledge of the chemical relations of food to respiration, would have allowed us to predict them with the utmost certainty. To the chemist they offer no new fact; and the only use they can have is in showing those

* *Valentin's Repertorium*, Jahrgang, 1843, p. 345.

whose studies do not enable them to appreciate other evidence, what reliance may be placed on the interpretations of chemistry in their application to the phenomena of animal life.

The very low point which the exhalation of carbonic acid reaches in sleep, is the combined result of the diminished activity of respiration and circulation during that state. In sleep, as is well known, the pulse falls, and the breathing becomes limited both in rate and amplitude; need must be, therefore, that the exhalation of carbonic acid diminishes in proportion. The knowledge of this fact furnishes us with an important rule of life for persons exposed to the various disorders that result from repletion; namely, never to indulge in sleep soon after a meal. To limit the action of respiration at the very moment when the materials that feed this process are being poured into the blood in large quantity, must, by protecting excrementitious matters from oxidation, inevitably tend to an accumulation of bile and lithic acid in the system. This explains the peculiar dangers of suppers in disorders of the class now alluded to, and shows how a single excess of this kind may, as practitioners have often an opportunity of observing, bring on a fit of gout or a bilious attack in persons predisposed

Of researches on the urine, by far the most important are those published in the *Lancet* by Professor Liebig. As these researches are not only the latest, but also supersede our former knowledge by new discoveries, they are the only ones of which I shall give an account. To discover anything new in the constitution of a fluid, the investigation of which has so long exercised the ingenuity of a host of eminent chemists, is in itself a proof of no small sagacity and practical skill. Now, in his paper in the *Lancet*, Liebig announces a twofold discovery of great physiological importance:— first, that lactic acid, which all other chemists have supposed to be an essential constituent of urine, does not exist in it; and, secondly, that hippuric acid, which was formerly considered to be peculiar to the urine of herbivora, is a constant and essential ingredient of that of man, and probably of other animals that subsist on a mixed diet. "All the urine," he says, "taken from individuals living upon a mixed animal and vegetable diet, contains hippuric acid, besides uric acid, and about the same proportion of both acids." I must refer my readers to the paper itself for the

evidence on which these statements are founded, contenting myself here with remarking, that it seems perfectly conclusive on both points. The author gives a simple process, by which hippuric acid may be obtained even from very small quantities of urine. He was led to the discovery of this acid in human urine, by finding, as Proust had done before, that in putrid urine of this kind benzoic acid is a constant product. He argued that the latter acid could not be an ingredient in *fresh* urine, since Ure and Keller have proved that crystallised benzoic acid becomes converted in the living organism into hippuric acid, and appears in the urine as hippurate of soda. Now, it being already ascertained that in the urine of the herbivora benzoic acid is developed by the putrefaction of hippuric acid, he inferred that in putrid human urine it must have the same origin, and that this urine also, in its fresh state, must contain hippuric acid. Direct investigation established the truth of this inference, as already stated. Concerning the source of this acid he offers the following considerations:—

"As far as our investigations into the composition of the aliments of man will allow us to judge, they contain no benzoic acid from which hippuric acid might have been formed; and as the urine of cows is invariably rich in hippuric acid, no matter whether the cows have been fed upon hay or upon beet-roots,—of which latter plant we know positively, from the results of several examinations, and from the experience derived from the manufacture of beet-root sugar, that they contain no benzoic acid,—we can come to no other conclusion concerning the presence of hippuric acid in the urine of persons living upon a mixed vegetable and animal diet, than that it is a product formed in the organism, to the formation of which their non-nitrogenous aliments give birth."

These several points being ascertained, the author was enabled at once to point out the true cause of the acidity of urine, about which there has been so much debate. He first lays it down in principle that the inorganic bases present in urine, such as potass, soda, lime, &c., have entered the organism through the medium of the aliments; a proposition which is, in fact, self-evident.

Now, in the ashes of meat, and of the flour of cereals and leguminous plants, these elements are found in combination with phosphoric acid, in the form of bi-basic and tri-basic phosphates,—salts which have an alkaline reaction. In animals fed on these

aliments, the urine should therefore be alkaline, if no acids were added to it by the living body. But the uric and hippuric acids, which are products of the vital processes, and are removed from the organism, together with the soluble phosphates, through the urinary organs, both possess the property of combining with the soda or potass of the alkaline phosphates, and acquiring in the combination a higher degree of solubility than they possess of themselves, at the common temperature of the body. "It is therefore obvious," as the author remarks, "that by the accession of these two acids, and by their action on the phosphates of soda, an urate and hippurate of soda must be formed, on the one hand, and an acid phosphate of soda, on the other; and that, consequently, the urine must acquire an acid reaction."

Another acid, the *sulphuric*, which is continually being formed in the living body by the oxidation of sulphur,—an element which is a tolerably large and a fundamental constituent of many kinds of food,—must also take its share of the alkaline bases, and further contribute to the same result; so that, in fact, the acidity of the urine is owing to the bases, which enter the economy as basic phosphates, being divided between the phosphoric acid they are originally united with, and three acids formed in the living body, namely, the uric, hippuric, and sulphuric acids. From this it would appear, that if, in addition to the basic phosphates, food be taken containing alkalies in combination with vegetable acids, the urine may become alkaline without the intervention of any other circumstance: for, in consequence of the oxidation which the acids of these salts undergo in the living economy, they all reappear in the urine as alkaline carbonates. Now, all vegetable aliments, without exception,—tubers, roots, greens, potatoes, turnips, &c.,—contain salts of this kind; and Liebig asserts, that after eating largely of vegetables, or of fruits, as strawberries and cherries, the urine is invariably alkaline. When they are partaken of in a certain proportion, he states that the carbonated alkali derived from them neutralises the acids that are present, and renders the urine neutral; when in larger proportion, it imparts to the urine an alkaline reaction: from which he infers that the acid reaction of healthy urine is purely accidental, and that urine of an alkaline or neutral reaction cannot be considered as a symptom of a diseased condition of the body. However this may be, all persons compe-

tent to judge in such matters will agree in the main with the author, in the following propositions:—

"First; that the analysis of urine, when made without respect to the inorganic salts, acids, and bases supplied by the aliments, teaches nothing whatever, and by no means justifies us in drawing therefrom any physiological or pathological inference. Secondly; that, from the nature of the ashes of the aliments, we are able to determine positively the inorganic constituents of the urine emitted. And, thirdly; that only when the amount and nature of these ashes have been distinctly ascertained, can we expect to derive from the analysis of the urine any correct information *with respect to the inorganic matters which have come to be present in it through processes of disease.* This, at least, is the *chemical method* of quantitative investigation."

Such is a very brief summary of the leading results put forward in this highly original and important paper. But the whole communication is replete with interest, and is characterised throughout by that ingenious application of principles to the interpretation of the phenomena of life, which so remarkably distinguishes all the productions of this eminent chemist, and makes them so rich in suggestions, as well for the practitioner as the physiologist. It well deserves a most attentive study at the hands of both.*

From this account of the chemical forms in which the materials of the body are discharged in their daily waste, we naturally pass

* The following extracts furnish a good illustration of these remarks:—"It will now be understood why the alkaline phosphates are generally absent from the urine of herbivorous animals, and also why, in certain cases, they may be found in the urine of these animals. If the nutriment of these animals contain no soluble phosphates, their urine cannot contain any, whilst if we add a certain proportion of grain to their food, the alkaline phosphates may be detected in their urine. Thus it is obvious, likewise, that the soluble phosphates in the urine of man are merely accidental constituents, and that by simply adding lime or magnesia to the aliments, and thus assimilating the constitution of these aliments to that of the food of herbivorous animals, the urine must become altered in its nature and properties. The knowledge of the influence which alkalies, magnesia, and lime, or acids, exercise upon the properties of the urine, or, in other words, upon the secretory process of the kidneys, in the healthy organism, is of the highest importance for the curing of diseases. I believe that there is now required only a small number of good and correct observations to establish a fixed rule for the remedies necessary in various cases. Future properly directed experiments will prove whether sanguification is absolutely dependent

on to the consideration of the various proportions in which these materials contribute to it, and of the phenomena which occur when this waste is no longer repaired by supplies of food. M. Chossat, a French physiologist, has lately undertaken to elucidate this subject by experiments, on a large scale. His plan was simply to starve a great number of animals to death, by partial or total privation of food, and to make accurate record, from day to day, of the principal phenomena that occurred in the course of the process. The results thus obtained are given in a work entitled, *Recherches Experimentales sur l'Inanition*; a work which appears to have gained great credit for the author in France, the Institute having conferred upon it the very high distinction of a Monthyon prize. To us, however, it appears at first sight as nothing less than wanton cruelty of the worst kind, to starve animals to death merely to have an opportunity of observing the phenomena of inanition, when these phenomena are, unfortunately, daily exhibited to us on a large scale, in the effects of a variety of wasting diseases, and, though more rarely, yet in all the simplicity of experiment, by the accidents of famine, shipwreck, and other forms of human calamity. Let us see whether or not there is anything in M. Chossat's results to lead us to abate the rigour of this first impression; whether the importance of the end be such as to justify the means.

The phenomena which engaged M. Chossat's chief attention were those which have reference to weight and temperature. As his victims were weighed every day, once at least, and at the same hour, he was enabled to ascertain very exactly the rate of diurnal

upon the presence of alkaline phosphates or not. We shall be able to determine whether weak solutions of alkaline phosphates are not the best solvents for uric acid deposited in the bladder, and likewise, what is the influence which aliments rich in sulphur, such as mustard, for instance, exercise upon the separation of uric acid in the bladder, in consequence of the formation of sulphuric acid. At any rate we may, by a judiciously selected diet, alter, with positive certainty and at pleasure, the nature of the urine; we may, without causing any injury to health, keep it alkaline for a long time by adopting a vegetable diet, and this is, certainly, the first condition necessary to insure the entire prevention of the formation of uric acid, as is the case with the herbivorous animals. By its combination with an alkaline base, uric acid must, in the organism, resolve itself into its ultimate oxygen compounds, with the same facility as other organic acids, if the physician prohibit all substances to be taken as food, which, like wine or fat, take possession of the oxygen necessary for the transformation of uric acid into carbonic acid and urea."

loss. The only point of any interest in these observations, was the rapid acceleration of this rate in the day or two immediately preceding death,—a period when common chemical agencies are beginning to resume their sway, and the enfeebled vital resistance is already giving up the body a prey to decay and dissolution.

In regard to the total loss, M. Chossat attempts to deduce a general proposition to the effect that, whether life during inanition be more or less prolonged, this loss, compared with the original weight of the animal, is always nearly the same in the same species. The average in warm-blooded animals he states to be 40 per cent. It is obvious, however, that such a proposition as this cannot be taken in an absolute sense, for the amount of this proportional loss must vary largely with the amount of fat previously accumulated; nearly the whole of which, as is well known, disappears in starvation. Thus, in the now well-known instance of the hog that remained alive for 160 days, buried beneath a cliff at Dover, and shrunk during that time from 160 lbs. to 40 lbs., the loss, instead of being less than one-half, was three-fourths of the original weight.* This is a point, in fact, which is so clear as to need no illustration.

M. Chossat made a great number of observations in order to ascertain in what proportions the various constituents of the body contribute to the general waste. The results he obtained are expressed in the following table, in which the loss sustained by each of these various constituents is set down in order, according as it stands above or below 0.40, which, it will be remembered, was the average loss of the whole body in M. Chossat's observations:—

Parts which lose MORE *than the mean* 0.40.		*Parts which lose* LESS *than the mean* 0.40.	
Fat	0.933	Muscular portion of stomach	0.397
Blood	0.750	Pharynx and œsophagus	0.342
Spleen	0.714	Skin	0.333
Pancreas	0.641	Kidneys	0.319
Liver	0.520	Respiratory apparatus	0.222
Heart	0.448	Osseous system	0.167
Intestines	0.424	Eyes	0.100
Muscles of locomotion	0.423	Nervous system	0.019

Many of these results, however, must be regarded merely as rude approximations; for it is plainly impossible to obtain with any

* *Transactions of the Linnean Society*, vol. xi., p. 411.

greater approach to exactness, the loss of weight suffered by many of the elements here specified; such as the blood, for example, the muscular portion of the stomach, and so on.

The next series of observations given, was made in order to ascertain the effect of starvation on the temperature of the body. Knowing, as we do, that the food is ultimately the source of animal heat, it was natural to expect that one of the first effects of starvation would be a fall of temperature in the animal. Observation has already taught us, as Liebig has well remarked, that a starving man is soon frozen to death; and that in gratifying our appetite for appropriate food, we obtain the best protection against cold.

In the animals starved to death by M. Chossat, the fall in temperature was uniform, but slow, up to the day before death; the total fall in the thermometer during this period being about 2°.5 *centigr.*; the daily fall about 0°.3 only; but on the last day, the thermometer fell rapidly from hour to hour, until the animal died, the temperature having sunk on an average 14° in the course of this day, and being at the moment of death, only 24°.9. As animals plunged in freezing mixtures generally die when cooled to this point, and as M. Chossat's victims perished with all the symptoms of death from cold, he was led to infer that the immediate cause of death in starvation is the failure of animal heat. The truth of this inference was fully established by the following experiment:—Several turtle-doves, at the very point of death, were placed in a vessel made for the purpose, and warmed by artificial heat. Before the heat was applied they were in a state of insensibility, with their bodies abandoned to the force of gravity, and without power to move; but as their temperature rose their senses and vigour returned; they began to flap their wings, then perched themselves on the edge of their stove, and next flew about the room and took what food was offered them. If they were kept warm for some time longer, most of them recovered, when well fed; but it is important to remark, that if the artificial heat was withdrawn too early, the food was not digested, and they soon died.

Such are the principal results of M. Chossat's investigations; the only ones, indeed, for which any importance can be claimed. Some of them are, without doubt, more or less interesting; but we must here repeat the question—Was it justifiable to subject hundreds of animals, endowed with great capacities of pain, to

the lingering tortures of death by starvation, in order to obtain them? Was it even necessary for the end in view? To the shame of this cruel physiologist be it stated, that there is not one of these results which might not have been deduced, scarcely one, indeed, which had not already been elicited, and that not vaguely, but with every needful precision, from the principles of organic chemistry, and the facts which accident and the observation of wasting diseases had already thrown in our way.

In most recent works on organic chemistry, but especially in that of Liebig, they are already clearly and explicitly laid down; and not as in M. Chossat's investigations, in the form of blind and empirical facts, but in that of deductions from principles established on a scientific basis, and giving the rationale of the phenomena.

Liebig's account of the phenomena of starvation has all that superiority over that of M. Chossat which science has over dry empirical fact without interpretation.

But, to enter into particulars, let us ask—Was it necessary to starve great numbers of turtle-doves to death, in order to ascertain that fat and muscle and its equivalent, blood, and other kindred matters, contribute most to the waste of the body in starvation? If accidental starvation and wasting diseases had never been, would not the phenomena of hybernation and the mode in which the carnivora are nourished have sufficed to establish all these points on a basis that could not possibly be made more sure? Did we require M. Chossat's experiments to learn that the fibrous tissues, and those structures of which gelatine, with or without large quantities of earthy matter, is the basis, suffer but small loss in comparison? For the chemist, as well as the physiologist, the small supply of blood these structures receive, would alone have been sufficient evidence. Every day observation, too, had prepared us for the fact, that the nervous centres shrink but little, and science had already offered an explanation of it.*

* In reference to these points, I cannot refrain from quoting the following interesting passages from Liebig's *Organic Chemistry in Application to Physiology and Pathology*:—

"In the progress of starvation, however, it is not only the fat which disappears, but also, by degrees, all such of the solids as are capable of being dissolved. In the wasted bodies of those who have suffered starvation, the muscles are shrunk and unnaturally soft, and have lost their contractility: all those parts of the body which

It certainly was an unexpected result, that the waste of these structures should be less than that of any others; the bones not excepted. According to M. Chossat's observations, this waste is scarcely appreciable. I cannot but think that there must be some fallacy here, and that the fact will ultimately receive an interpretation, which will refer the maintenance of weight, in some part, at least, to the substitution of blood or other interstitial matter for wasted tissue. At least, there can be no doubt that the brain does very sensibly waste in the emaciation of disease. The manifestly shrunken state of the organ in the bodies of those who have died of diabetes, of cancer of the pylorus, and other diseases that kill by inanition, is almost sufficient evidence of it. But if this left any doubt on the point, it would be entirely removed by the more precise observations of M. Foville, which, on account of their intrinsic interest, I shall here quote entire. This author, in a recent work, which I shall have to pass in review in the sequel, makes the following remarks:—

"It is generally admitted that the cerebro-spinal system does not participate with the body in changes of volume corresponding to those which the latter undergoes in passing from a state of plumpness and muscular development to the wasted state which chronic diseases lead to. This opinion is without foundation.

"It is easy to satisfy one's self of this, on comparing the brains of fat and robust persons who have died of acute diseases, with those of others who have died in a state of marasmus. In the former, the convolutions of the brain, the lamellæ of the cerebellum, the peduncles, the pons, and even the spinal cord itself, show a degree of volume and roundness of form which is not met with in the latter. In these, in fact, the convolutions and lamellæ are shrunken; the broader inter-spaces between them are filled with a large quantity of serum; the peduncles, pons, and spinal cord are more angular in their forms, while their whole mass is diminished. The two offer, in one word, differences corresponding to those

were capable of entering into the state of motion have served to protect the remainder of the frame from the destructive influence of the atmosphere. Towards the end, the particles of the brain begin to undergo the process of oxidation, and delirium, mania, and death, close the scene; that is to say, all resistance to the oxidising power of the atmospheric oxygen ceases, and the chemical process of eremacausis or decay commences, in which every part of the body, the bones excepted, enters into combination with oxygen."—pp. 26, 27.

which the limbs themselves exhibit in their extremes of development and leanness; a practised eye cannot be deceived in it.

"These variations in form and volume do not, however, suffice to prove that the cerebro-spinal system contains more or less fat, according to similar variations in the general plumpness; but we may easily obtain direct proof of this, by suspending in a vessel filled with spirit, the brain and spinal cord of a robust and fat man, killed by accident; and in a similar vessel, likewise filled with spirit, the same organs of a man dead of emaciation.

"In the course of two or three days, the whole surface of the former will be found covered with fat in crystalline needles, which become so abundant as entirely to hide the structure beneath. In the other vessel, nothing of the kind occurs, as water is the only principle the nervous mass it contains yields to the spirit."

After making the interesting remark, that the brain of the *cetacea* so abounds in fat as to float in water, M. Foville adds—"That very often the peculiar aspect of the brain of aged men is due to emaciation; but that this must not, however, be confounded with the senile or morbid atrophy of this organ, and which has its own characters."*

But by far the most interesting result of M. Chossat's investigations—to which I now return—is that which shows that the immediate cause of death from starvation, is the failure of animal heat. This is a fact which has many important practical applications in the treatment of febrile diseases of long duration; but even in this fact there is nothing that might not have been strictly deduced as a logical inference, from the known chemical relations of food to respiration; nothing indeed that had not already been clearly laid down in principle, and almost in so many words, in various chemical works in which these questions are considered. In support of this statement I may confidently appeal, more especially to Liebig's work already quoted, and to various parts of M. Dumas' paper on "Neutral Azotised Matters," of which an analysis has already been given in these pages.†

After these remarks, I need scarcely add that I am at a loss to know on what scientific grounds the French Academy thought fit

* *Traité complet du Systéme Nerveux Cerebro-spinal*, pp. 120 et seq.; M. Foville, 1844.

† "Memoires de Chimie," see particularly pp. 340–41.

to confer so high and valuable a mark of distinction on this memoir. For thus giving, with so little to justify them, such an effectual encouragement to cruelty of the worst kind, they deserve the grave rebuke of all humane men. I am not one of those who entertain what may be held to be maudlin and sentimental objections to experiments on living animals. But while freely admitting that such experiments are justifiable, where we are warranted by previous and well-founded induction in the expectation of results of importance to the well-being of man, attainable by no other means, I the more feel bound, on the part of all true lovers of science, to enter an indignant protest against such wretched cruelties as these, undertaken almost at random for the chance of what may turn up, and for the production of phenomena which accident and disease are daily offering to our observation on a large scale, and into the nature of which science has already given us the clearest insight. I want words to express my own abhorrence of these practices, and my deep sense of the guilt of those who resort to them. The frequency of them in France, and the shocking levity with which they are undertaken, is the great blot on the present character of physiological science in that country.

I shall not weary my readers with any account of another series of almost equally cruel experiments, undertaken by the same physiologist with a view to determine the effects of a diet of pure sugar. I pass them over with the greater satisfaction, since the author of them has already received on their account a well merited chastisement from a more powerful arm than mine.*

To complete this review of the recent progress of organic chemistry, it only remains for me to briefly notice Liebig's remarks on the kindred processes of fermentation, putrefaction, and decay,—processes which, in one form or another, play an important part in many both healthy and morbid changes in the living body, besides their allotted task of resolving it after death into the chemical elements out of which it originally came. His observations are quite decisive in showing the purely chemical nature of the agency which the ferments and other bodies in a

* See "Observations on Organic Chemistry," by Professor Liebig; *Lancet*, June 29, 1844, p. 437.

state of change play in these processes. He well remarks, in objection to those who ascribe an essential part in them to microscopic beings, "If the fungus be the cause of the destruction of the oak, and the microscopic animal the cause of the dissolution of the dead elephant, to what cause then are we to ascribe the putrefaction of the fungus and the microscopic animal after their death? They also ferment, putrefy, decay, and disappear gradually, first in the same manner as the oak and the elephant, and in the end furnish the same products."

This single objection decides the question, and is quite unanswerable. But although Liebig thus shows the absurdity of the notion that animalcules play any essential part in putrefaction, he nevertheless admits that they greatly accelerate the resolution of dead matter into more simple chemical elements, and thus hasten the process. This is clear enough, since all animals are ever actively reducing, by the function of respiration, the complex atoms on which they feed into carbonic acid and water, which are two of the last products of decay.

These observations are so conclusive that I may dispense with giving an account of the otherwise interesting experiments of Helmholtz, who succeeded in infecting organic mixtures with an active state of putrefaction, by introducing into them other putrid fluids by a process of endosmosis, whereby all living organisms were effectually shut out.*

I cannot close this part of my Report without expressing my regret and humiliation at having, through its whole course, scarcely once had occasion to speak the name of an Englishman. It is true that in this province we have one or two individual names, of which we may well be proud; but we shall look in vain for any thing worthy to be called a *school* of organic chemistry in this country, and still more, for anything worthy to emulate the noble schools of which Dumas and Liebig are the heads in France and Germany. I earnestly hope that we shall not long allow ourselves to be thus outstripped in this noble rivalry of nations, but that our own countrymen will strive to share in the glories that are

* See Erdmann and Marchand's *Journal für Praktische Chemie*, No. vii., 1844, p. 429: "Ueber das Wesen der Fäulniss und Gährung," Von Dr. Helmholtz. In many respects an interesting paper.

yet in store for men of genius, in the bright career which these illustrious chemists have opened to us.*

I may here be allowed, in particular, to express a hope, that in the changes now in contemplation in the education of members of our profession, the teaching of chemistry on a grand scale, both in theory and practice, may be made a chief object. In the education of the physiologist, chemistry should be put on a level with anatomy, *since chemical change is, at least, co-ordinate with structural form as a condition of life.* Of the two, I am not sure whether a knowledge of the former does not give us greater insight into the most remarkable of these phenomena, seeing the intimate and material relationship which the consumption of matter bears to the various forms of force which the animal has at its command. I am deeply convinced, that until these truths are clearly seen and acted upon, it will be vain to hope for any great advance in medical science.

Before I pass on to the next division of my subject, I must refer to some recent observations on the physical phenomena of the circulation and of absorption. Those to which I chiefly desire to draw attention will be found in a paper by Dr. Marshall Hall,† on "The Circulation in the Acardiac Fœtus;" in another, by

* It is deeply to be deplored that this science, and the sciences generally, receive so little encouragement in this country at the hands of the State. It surely would be both wisdom and economy in a great nation like ours, which has taken to itself the high task of subduing every power, and appropriating every product of the earth to man's use, to set aside and endow a great body of eminent men for inquiry into Nature. A vast step will be gained in civilisation when our legislators shall come to see in all the clearness in which this truth must ultimately appear, that in this direction lies all future extension of our power. It is surprising that such a truth should still be but vaguely and obscurely apprehended, when the intimate connection between the most abstract discoveries in science and the greatness of nations, is daily set before us in such a striking point of view in the connection of astronomy with navigation. National astronomical observatories are kept up, because without them commerce must cease, and the ocean become what it once was—a trackless waste. But because the arts and agriculture still go on, and even advance on their present purblind methods and by the help which science already gives them, would it answer the less to institute great chemical observatories also? The works of the laboratory of Giessen are a sufficient answer. Already, on the faith of discoveries made there, a fleet of British merchantmen are floating their freight of guano to our shores; a freight which, before another year is over, will bring its return in corn and oxen.

† *London and Edinburgh Monthly Journal of Medical Science*, June, 1843.

Spengler,* on "The Strength of the Arterial Stream;" in Dr. G. Burrowes's excellent Croonian Lectures on "The Circulation in the Brain;" and in some papers by Mr. G. Robinson, in the *Medico-Chirurgical Transactions* and the *Lancet*, on "Venous Absorption." All these papers will be found worthy of an attentive study, and I regret that I have not room for more than this bare mention of them.

I would only remark, that Mr. Robinson's papers are distinguished by great clearness of ideas on the particular subject in hand, and that his observations on absorption, especially, will, if confirmed, tend to give great precision to our views of this process, and to fix more exactly than we have hitherto been able, the conditions by which it is regulated. These are points of great practical importance, because on our knowledge of these conditions, so far as they are under our control, must depend the proper application of remedies in a great variety of cases.

It is to be hoped that our increasing knowledge of these subjects will soon enable us to dismiss such vague phrases as "stimulating the absorbents," and the like; a phraseology which belongs to the infancy of science and a barbarous age, but is quite intolerable in the present day.

I now pass on to the other great division of my subject, namely that which relates to the anatomical form of the structures of the body, and to the functions of these structures considered in the sense of vital endowments. From the vast number of observations in this wide field, I am compelled to restrict myself to a selection of the more interesting and important. Many things must be omitted from this selection, which of right should find a place in a report of progress. In excuse for this I must plead my own want of time, and the limits of the space allowed me in these pages,—limits which, I fear, even with the reserve which circumstances have imposed upon me, I may still be tempted to transgress, to the exclusion of more important matter. I feel it necessary to say thus much, for the satisfaction of those who may be disappointed at finding no notice of their labours here. I may also add, that a great number of observations, of various kinds, although both novel and interesting in themselves, have been left out designedly;

* *Müller's Archiv*, 1844, heft i., p. 49.

for the object I proposed to myself in this Report, from the outset, was, not to give a complete and detailed analysis of the labours of the year in the form of dry catalogue, but rather a connected, and, in so far as in me lay, a critical view of such among them as touched on great questions in physiology, or seemed striking from their novelty and individual importance; venturing here and there, while surveying the past, to point out in what direction lie our hopes for the future.

In my choice of subjects I have also uniformly given a preference to those which illustrate the principles of investigation which should guide us in physiological inquiry. By the adoption of these restrictions, a large mass of matter was at once excluded from this Report. In the subjects belonging to this division of it, almost the only labours of the past year that answer the conditions just named, are two or three works on "The Anatomy and Physiology of the Nervous System," Matteuci's striking discoveries on "The Electric Phenomena of Animals," and some recent investigations into the nature of certain functions connected with reproduction.

The chief part of what space remains to me will be given to the consideration of these subjects. In order to enter upon them the sooner, I shall, therefore, pass very rapidly over some other objects of minor importance, beginning with the blood, as being the first step in the passage of the food from the state of raw material into that of organised forms.

In the *Zeitschrift für Rationelle Medicin*, Bd. i., s. 238,* there is a very interesting paper, by Scheerer, on the cause of the difference between the colour of arterial, and that of venous blood. This paper contains a great number of observations, all tending to confirm the opinion already advanced by Dr. John Davy, as also by Henle and Nasse, that this difference of colour depends rather on physical than on chemical conditions. Scheerer is of opinion that it is related to differences in the form of the blood-corpuscles, affecting the manner in which light is reflected by these bodies. His researches seem to render it probable, that the bright colour of arterial blood is connected with a bi-concave, the dark colour of venous blood, with a more globular form of the corpuscle. In support of this, he has ascertained by the microscope, that salts

* See also *Schmidt's Jahrbucher*, No. ii., 1844, from which the account in the text is taken.

and oxygen, which alike have the property of communicating an arterial colour to venous blood, have this effect in common: that both, namely, impress a very marked bi-concave form on the blood-corpuscle; whereas, water and carbonic acid, which give a dark hue to blood, cause the corpuscle to swell out and become bi-convex. That the colour of arterial blood is not the effect of mere chemical change in the colouring matter in consequence of its uniting with oxygen, would appear from the fact, that if a stream of oxygen be passed through venous blood previously diluted with water, no change of colour ensues, but the fluid remains dark as before. The same is the case if, instead of oxygen, salts be added to the diluted fluid; the reason being, according to the author, that the blood-corpuscles retain the globular form in consequence of their having imbibed water by endosmosis. In conclusion, he points out some other conditions, which probably have a share in causing the change of colour which the blood undergoes in its passage through the lungs.

The next observations that claim our attention are taken from a monograph by E. Horn, and relate chiefly to the *genesis* of the blood, and especially to the relationship which its various corpuscles bear to one another.* The lymph-corpuscles he regards as the true development-cells of this fluid; he finds them extremely abundant in blood taken about an hour after a meal, as also in the blood of pregnant women. The coloured corpuscles are, according to the author, the immediate offspring of these. In the course of development each lymph-corpuscle becomes a parent cell, containing a progeny of several smaller ones, these last being the embryo state of the coloured corpuscle.

These observations agree, in the main, with some formerly published by Dr. Martin Barry, and, according to Valentin, they have also been confirmed by Remak. Besides these two kinds of corpuscle, Horn describes, under the name of nucleated cells, other bodies of round or oval shape, and from two to four times smaller than the developed blood-corpuscle. These bodies are exceedingly numerous in the blood of birds, amphibia, and fish, but less so in that of mammals.

* *Das Leben des Blutes und die Gesetze des Kreislaufes, nach neuen Untersuchungen bearbeitet*, Wurzburg. See also *Valentin's Repertorium* for 1844, from which the account in the text is taken.

Finally, he recognises other bodies again, of still smaller size, and of yellow or reddish colour, swimming free in the *liquor sanguinis*. The author looks upon these last as the first solid forms arising out of the liquor sanguinis, and has named them "primordial nuclei."

The work also contains a great number of observations on the formation of the blood-corpuscle in the adult, on the seat of the colouring matter of blood, and on the chemical and physical characters of this fluid.

In *Müller's Archiv*, Meyer has described some singular varieties of form in blood-corpuscles, found in the blood of various reptiles,—varieties which have some interest in their bearing on the mode of development of these bodies. Thus, scattered among the normal corpuscles, were some provided with tails, others spindle-shaped, and others, again, contracted in the middle and swelling out at each end in a vesicular form, so as to offer some resemblance to dumb bells. This last form, in particular, acquires interest from the fact, that from the observation of similar, but more definite appearances in cells of various kinds; in the blood-corpuscle, namely, in the transitory cells of the yolk, and in those of which the simplest infusoria are made up, Dr. Martin Barry has been led to the theory, that by far the most general, if not the only mode in which cells multiply, is by fissiparous division. According to Dr. Barry, the process begins in the nucleus of the cell, or, as he calls it, the hyaline body, and is afterwards extended to the whole cell.*

The minute structure of the spleen still continues to baffle the skill of anatomists. Among others less distinguished, MM. Flourens and Bourgery have made a very laborious examination into the anatomy of this organ, but without any result of much novelty or importance. M. Bardeleven has endeavoured to gain some insight into the nature of its function, by watching the consequences of its extirpation in dogs. He found, as others had done before him, that those animals that survive the operation, retain their health and are, to all appearance, exactly as before. He even obtained the same negative result in animals from which he removed the thyroid body also. In neither case did any function

* *Jameson's Journal*, 1843.

appear to suffer. In particular, he ascertained that there was no diminution of the venereal appetite or procreative power. In no case did he witness anything like an attempt to form a new spleen, as Meyer, of Bonn, pretends to have done.

I regret that I can give no account of Mr. Simon's prize essay on "The Anatomy and Functions of the Thymus," in consequence of not having yet had an opportunity of reading it.

In *Müller's Archiv*, (1843, heft. iii.,) are two papers on "The Anatomy of the Liver," by Weber and Kruckenberg, which contain a good account of the vascular element of this organ. They point out, as its leading characteristic, the immediate breaking up of large vessels into minute capillaries, forming a network throughout the liver;—a description which accurately agrees with that of Mr. Bowman. The only novel part of their papers concerns the biliary ducts. Weber distinctly asserts that these do not terminate in blind ends, but anastomose with one another, so as to form a similar capillary network to that of the blood-vessels with whose meshes they are interwoven. Strange to say, they neither of them make mention of the proper cells of the liver, which their mode of examination, namely, by injection, had probably hidden from them. They both agree in stating that there are no such things as distinct *acini* in the liver, separated from one another by cellular tissue, a statement in which they are again in agreement with Mr. Bowman, but which Müller endeavours to controvert in some critical remarks appended to their papers.

On other parts of anatomy there is nothing that need further detain us from the more important labours which concern the nervous system, and the other subjects to which the remainder of this Report will be given.

Various motives still continue to render the nervous system and the great function of reproduction, the favourite subjects of investigation with physiologists. The brilliant light already shed by the microscope on the most obscure parts of the physiology of reproduction, with its rich promise of further illumination, might well give to this subject more than common attractions for the

student, ambitious of distinction. But, in addition to this, the revival of the discussion touching the relation of the periods of *rutting* and menstruation to the spontaneous ripening and shedding of ova, has given all that activity to the investigation of this subject, which the introduction of an important theory invariably communicates to every branch of natural inquiry.

In the anatomy and physiology of the nervous system, the impulse given by the brilliant discoveries of Bell, and more recently by those of Marshall Hall, Flourens, and others, is still manifest in the ardour with which numbers of eminent men follow out, in various directions, the paths which these have opened. As yet, however, it is evident that in this branch of knowledge, discovery is only in its infancy. When we contemplate the vast number of problems as yet undetermined that here wait for solution,—problems whose data palpably lie within the limits of precise determination, and which already shape themselves out in the form of definite questions,—it is evident that this will long continue to be the most stirring field of speculation in the whole domain of physiology.

What subject, indeed, is there in the whole range of human inquiry, so fit to excite the imagination and rivet the attention of a speculative being, as the construction and laws of action of those structures, wherein mind is so mysteriously blended with matter, and that union of will with mechanical force is effected, which, while it has no parallel in nature, alone gives to man and animals their command over the world without?

During the past year, many physiologists of high name, some of whom have already earned distinction in the same field, have, with various objects in view, and with various success, devoted their time and energies to several branches of this difficult inquiry. Many reasons, however, deter me from attempting anything like a complete analysis of their labours. Of these reasons, the following will, I doubt not, be deemed sufficient:—

In the first place, several of the works in which these labours are set forth, are loaded with a vast mass of dry detail, which, from being by its very nature not susceptible of condensation, and from having no present connection with physiological speculation, any attempt to present, in the form of abstract, would be alike wearisome and unprofitable. In the next place, it will be

readily understood that descriptions of anatomical connections, so minute and intricate as those of the more complicated parts of the nervous centres, are utterly unintelligible without the help of plates. Indeed, even when the descriptive power of the anatomist is illustrated by the pencil of the artist, it is often extremely difficult to gather the meaning of the author. To transfer such descriptions to these pages, therefore, would mislead rather than inform, and tend to retard science instead of advancing it.

But a more weighty reason still, and one that of itself would justify me in the course I take, is that the authors to whose investigations I shall have to refer—physiologists of high repute, and of almost equal authority in such matters—are often in direct contradiction with one another, touching points of the most elementary kind and of cardinal importance. Thus M. Foville, whose name has acquired an European celebrity from his researches in this branch of anatomy, teaches, in his description of the spinal cord, that through the whole length of this structure there is a decussation of its anterior columns, so that ultimately the whole of the fibres of the right side of these columns are found to have passed to the left, and *vice versa*. In the upper part of the cord, between the pyramids, some such crossing is very generally admitted, and is, as one would fancy, very obvious to sight. M. Foville states, that the same thing may be shown through the whole cord by a very simple anatomical manipulation. He states further, that in the act of crossing these fibres dip inwards, so as to become lateral, and at the same time so entirely change in aspect as from being white to become grey. Still more than this, M. Foville adds, that at his suggestion M. Gruby has examined these parts most carefully with the microscope, and has entirely confirmed every one of these observations. Moreover, to illustrate the text, several figures are given, in which all these details are exhibited with such clearness and distinctness as would seem to leave no doubt of their being true to nature. Among M. Foville's plates there is certainly no figure more clear and defined than that which exhibits the decussation of the anterior columns. If this figure is not to be trusted, one really cannot know what faith to put in the rest. What shall we say, then, when we find Dr. Stilling,—who claims the greatest accuracy for his observations, and who has examined the cord throughout most minutely with

the microscope,—stating, that if there be two points in its anatomy that may be considered absolutely clear and certain, they are these:—that there is no decussation of the anterior columns, and that nowhere are the white fibres found passing into grey. Hannover, again, fully confirms these statements by independent observations; and the apparent truthfulness of his plates, and the fact of his having received a prize for his investigations from a body well qualified to pass judgment in such matters, would seem a sufficient guarantee for his trustworthiness.

Now, when men of the first authority differ on such a cardinal point as this, and one, as it would seem, so easy of determination by methods at ready command, one is really at a loss to know what to think of any of their observations. The fact should certainly impose on us extreme caution in admitting any such into a report of progress, unless they come recommended by various and weighty support of other kinds.

For these several reasons, in my review of the progress of this branch of anatomy and physiology, I shall, as a general rule, only touch on such observations as seem to be established on good grounds, or to involve doctrines of cardinal importance; reserving, however, the privilege of extracting, now and then, any isolated fact that may appear to be of more than common interest.

Of the works I shall have to pass in review, that of M. Foville, *On the Anatomy of the Brain and Spinal Cord*, is by much the most voluminous; and, as far as evidence of years of unwearied labour goes, although this by no means constitutes its highest pretensions, it comes before us with stronger claims to our notice than any of the others. Unfortunately, however, for these same reasons, the difficulties in the way of an analysis of his researches are greater in the same proportion, and, in fact, under the circumstances, quite insuperable. To attempt to give anything like even a cursory view of the whole, within the narrow limits of this report, would be to do the author great injustice; and those who desire to obtain it, must refer to the book itself. I must content myself here with a sketch of the most general and important of the results he has arrived at, offering, in conclusion, a few remarks on his methods of investigation, with a view to determine the degree of confidence to which they entitle his researches.

The decussation which M. Foville describes as taking place between the anterior columns of the cord, through the whole length of this structure, has already been spoken of. In addition to this, it is only necessary to state that, with most other anatomists, he divides the cord into three principal fasciculi,—the anterior, lateral, and posterior,—each giving origin to its own particular class of nerves; those from the posterior being exclusively sensory, those from the anterior exclusively motor.

It is right, however, to add, that those who will take the trouble to consult the work itself, will find in it a most accurate description of the cord in its external form and relations, and, as far as the methods of the author carry him, a most minute and elaborate investigation into the connections of its white and grey elements; into the form and direction of its commissures, and the ultimate relations of the roots of the nerves to these several parts. It is evident from the details into which the author enters, that he must have spent a vast deal of time and labour on this investigation, and must have subjected a countless number of specimens to careful and various examination.

Touching the general idea to be formed of the brain and cerebellum, the most prominent view put forward by M. Foville,—and it is evidently one to which he attaches great importance, as it is brought up again and again in the course of the work,—is, that these structures are to be considered as the ganglia of the nerves of special sense. He describes himself as originally indebted for this idea to M. de Blainville, who has long taught that the brain and cerebellum are to be looked upon as ganglia; differing from ganglia in general, however, in being without external apparatus. (*Sans appareil exterieur.*) But, in this particular, M. Foville regards M. de Blainville as fundamentally wrong; and he labours to show that the anatomical relations of these ganglia to the nerves of special sense are, in all essential conditions, perfectly analogous to those of the ganglia of the sensitive roots to the spinal nerves; that, like these, the nerves of special sense, also, arise from the central nervous axis by two roots, and that it is on one of these roots only that these ganglionic enlargements are developed. I must refer to the work itself for the anatomical evidence on which this view is sought to be established.

As to the originality of the idea, it is one that, in slightly

varying shapes, has long been current among physiologists of all countries; and so far from there being in it anything to wonder at, still less any ground of merit, it is one that, in some sense or other, must naturally suggest itself to every one at all versed in comparative anatomy, by the division of the encephalon, as we descend the scale of vertebrata, into a series of lobes or ganglia, each appropriated to its own nerve of special sense, and developed in exact proportion to the perfection of the organ which this nerve goes to supply.

But, after all, it may well be questioned whether the use of this language in its present vague sense be not, as the French so well express it, "*se payer de mots*,"—to substitute a phrase for real knowledge. To call the brain or cerebellum a ganglion,—a name, by the way, in itself of trivial meaning,—brings but small enlightenment while we are still in such profound ignorance of the attributes of the simplest organ of this class.

The great anatomical fact, in regard to the brain and cerebellum, which it appears to be one of the objects of M. Foville's work to establish, is that both may be separated into two grand elements, fundamentally different in character; namely, a central nucleus, formed upon, and constructed out of, the great central axis of which the cord is the continuation, and a covering to this nucleus, enveloping it in the form of a cap, and consisting of the convolutions and the fibrous expansion on which the cortical matter of these is laid down. It is in the brain, especially, that this admits of the most complete demonstration. The author gives a method of effecting this separation, and numerous figures of the two parts thus divided, which in the plates certainly look very definite and shapely, as if they were true and genuine anatomical, and not artificial elements of structure. The peripheral part, he labours to show, has with the nucleus only two principal physiological connections; one, by a broad central mesh of fibres, which comprises, in fact, the whole mass of fibres radiating from the great central axis to the convolutions; the other, by the convolutions themselves, which, in one point of their circumference only, are intimately united to the nucleus at the border of the perforated space—a region to which M. Foville attaches extraordinary importance, and which he regards as the grand centre and rendezvous of the most important elements of these structures. Thus, for instance, by

the union here mentioned, the cortical membrane is brought into perfect fusion with the origins of the great cerebral nerves, the optic, and the olfactory.

In separating, therefore, the nucleus from the external cap, the only two genuine anatomical connections that have to be severed are, according to M. Foville, the central mesh of fibres, and the junction of the convolutions with the perforated space. The central mesh, which is very broad, and springs from a curved line along the whole convexity of the nucleus, includes fibres from the posterior and lateral columns of the cord, and also nearly the whole of the anterior columns, which pass up almost entire from the central axis to the convolutions in this assemblage. The fibrous element of the connection between the convolutions and the perforated space is, on the contrary, wholly furnished by the posterior columns. The precise way in which this connection is established is somewhat difficult to apprehend; but as far as can be gathered from the description, the most important feature of it is, that the fibres from the posterior column here split into a double membrane, which incloses the convolutions in the space between its layers. The external layer constitutes the outermost white stratum, first well made known by M. Baillarger, and more recently accurately studied by Lelut; the inner one is here described, for the first time, by M. Foville himself, and must be added to the series given by M. Baillarger, whose anatomy of these structures, however, with this important addition, our author adopts entire. His description of this layer is very minute and circumstantial, and as this, if confirmed, must prove to be one of the most interesting results of M. Foville's labours, I shall give it in his own words.

Speaking of this layer in its relation to the stratum of cortical matter, he says,—"The former follows, within, all the folds of this stratum, in the same way in which the pia mater follows them without. The pia mater throws off from the surface corresponding to the cortical matter numberless vessels, which penetrate perpendicularly from without inwards; the deep layer of fibrous matter which we here propose to consider as an essential element of the cortical stratum, throws off likewise from its surface processes of nervous matter of extreme tenuity, which penetrate the cortical stratum from within outwards, in a direction the exact reverse of

that of the vessels. Thus, according to this view, four layers of white and three of grey matter constitute, by their assemblage, the cortical membrane of the convolutions of the brain and of the lamellæ of the cerebellum."

This account would seem to agree pretty well with Hannover's statement, that in the layer of white substance immediately below the grey, the fibres run parallel to the surface; unless, however, this should be found to apply to other fibres from the central mesh already described, which, in certain regions, also follow a horizontal course.

In the cerebellum, the analogue of the white membrane here described as lining the convolutions of the brain on the inside, is formed, according to M. Foville, by a membranous expansion, chiefly and jointly furnished by the roots of the seventh, or auditory, and fifth pairs of nerves. This expansion, he says, spreads itself out from the very substance of these nerves, in the same way as the retina from the optic nerve, and it lines the whole extent of the cortical layer of the cerebellum, in the same way as the retina lines the globe of the eye.

This, again, is one of the most remarkable results of M. Foville's labours, and, if found to be true, cannot fail to prove one of the highest possible interest in the physiology of these structures.

In concluding his account of the anatomy of the brain, he thus sums up the principal relations here passed in review:—

"By its circumference, therefore, the cortical layer is attached to the cerebral nerves and to the posterior column of the cord, and the elements it receives from these nerves and from this column, are continued over its whole extent.

"In its central parts this same cortical layer is penetrated from within outwards by fibres, which are continuous with the anterior column of the cord. The manner in which these latter fibres are combined with the cortical layer, is different from the mode of combination between this same layer and the fibrous elements it derives from the posterior column.

"These last spread themselves out into membranes of extreme tenuity over the whole extent of the cortical layer. Of this layer they are constituent elements.

"The parts derived from the fasciculated region of the peduncle, after having undergone all their transformations in the central

nucleus,—in the great radiating layers of the hemisphere, and, finally, in the processes furnished by the convex surface of these radiating layers,—penetrate the cortical layer by their extremities at the summit of the convolutions. They represent a sort of sheaf, whose ears, only, belong to the cortical layer. The sheaf itself may be divided into a considerable number of layers placed side by side, holding by one end to the summit of the convolutions, and passing in the other direction, by a course more or less oblique, to the great radiating fibres of the internal face of the hemisphere."

In addition to the details by which these cardinal relations are sought to be established, the work contains an elaborate description of the more minute and intricate fibrous connections between various parts of the brain and cerebellum; a minute account of the relations, external and internal, of the ventricles; and a very interesting study of the convolutions, in which, although there is a good deal attempted to be laid down, that seems forced and artificial, yet there is also much that is both novel and instructive.

M. Foville's researches into the origin of the cerebral nerves, also, although, for reasons already assigned, they can find no place here, are made with praiseworthy care and minuteness. They deserve the greater consideration, because, from the greater simplicity of the inquiry, it is less open to sources of error from defect in the methods and instruments of investigation. This and other reasons incline me to believe, that these researches will eventually be found to be the most important of M. Foville's labours. This is a branch of anatomical inquiry which has not hitherto been followed up with that constancy and perseverance which might have been expected from the rich promise it holds out to the physiologist; for, from the perfect knowledge we now possess of the functions of nerves, it is evident that in tracing them back to the particular regions from which they emanate in the central organs, we have one of the most trustworthy clues within our reach, to guide us in our inquiry into the attributes of the latter.

Among the more isolated observations scattered through the work, these few remarks on the pituitary body are worth quoting:—

"That which most strikes the observer in the pituitary body, is its situation in the central part of the base of the skull, in a case lined by the dura mater, and protected by marginal folds of the same membrane.

"The pituitary body is by its situation, and by the structure of the walls of its case, more protected from injury than any other part of the brain. An organ which nature has placed in such conditions ought, by the fact, to be an organ of great importance."

The author adds in a note, that having made numerous observations on the state of this body, it has often appeared to him diseased.

The leading impression left on the mind by an attentive study of M. Foville's work is, that in relation to the more minute and intricate parts of this anatomy, his methods are gross and coarse when viewed in connection with the known minuteness of the objects, and the extreme softness and delicacy of the tissue under investigation. These methods are not of a character to satisfy the requirements of anatomists of the present day, with their recent acquirement in the microscope in its present perfect state, of what may almost be called a new sense, and with their more enlarged basis of inquiry. What we have already learnt of the anatomy of nervous tissue by these means, is sufficient to show, not only that such procedures as those employed by M. Foville must, in a vast number of cases, be quite inadequate to lay open the true elementary relations of structure, but that they must often do violence to connections of perhaps the first importance, and thus lead us into serious error. This is a conclusion which may be drawn with absolute certainty from our present knowledge.

I trust, however, that in the observations I am here making, I shall not be misunderstood. It must not be lost sight of, that we here have the fruit of twenty years' labour, bestowed upon a favourite subject, and with a degree of constancy and devotion but too rare in these days;—by an observer, too, of remarkable talent, a practical and skilful anatomist, and having unlimited opportunities at command. We can scarcely estimate too highly the claims on our favour and attention which these circumstances bespeak for them. Whatever may be the ultimate fate of these researches,—whether to take a permanent place in the science, or to give way to the results of more intimate and searching methods,—no one can refuse to admit that they form a large and valuable contribution to our knowledge. But highly as we may estimate these researches as a whole, it is requisite, for the interests of science, to add, that, for reasons already assigned, we cannot show too much caution in admitting any demonstrations of the more

intricate parts of this anatomy, until they are ratified by the work of finer and more delicate instruments, or shown to have an especial warrant in analogy, or, still more, some essential connection with living phenomena and function. The absence of all elucidation from the two last sources seems to me to be the defective point of M. Foville's work. To find no reference whatever to embryology and comparative anatomy, is a great disappointment in these days, when these two sources of information are in general so largely drawn upon in similar works, and with such great profit to the inquiry.

The next works to be noticed are those by Drs. Stilling and Hannover, already cursorily mentioned, as containing the results obtained by these anatomists in their first attempts to make out the structure and relations of various parts of the nervous system, by help of the microscope. The account here given of them must necessarily be brief, as, without the plates which illustrate them, the greater part of the details of which they are made up are absolutely unintelligible. The methods employed, and the objects in view, were somewhat different in the two cases.

Dr. Stilling's investigations were confined to the spinal cord and *medulla oblongata*,—already, however, a sufficiently wide field. His mode of examining these structures was this:—to cut from a variety of specimens, *previously hardened by being steeped for some time in spirit*, extremely thin slices, both lengthwise and crosswise;—to lay these without covering on a glass, and examine them with great care under a low power. With a keen razor, and with the skill soon acquired by practice, Dr. Stilling states that it is very easy to obtain slices so thin as to be perfectly translucent, showing, as they lie on the razor, the bright lustre of the blade beneath. In these slices the course of the fibres can, for the most part, be distinctly followed, and their principal connections traced.

The following are the chief points of physiological importance in the author's account of the structure of the cord, as far as this can be understood without his plates, which I may remark, by the way, are extremely beautiful:—

His examination has led him to recognise in the cord three principal anatomical elements, namely, longitudinal fibres, transverse fibres, and certain corpuscles which, on account of their specific form and distribution, he names "spinal bodies."

The longitudinal fibres are of two kinds, white and grey, which, except in colour, only differ from one another in that the grey are softer than the white. According to the author, these two kinds of fibres form respectively the white and grey columns of the cord; for, in contradiction to former observers, he affirms that the grey matter of the cord, as well as the white, is entirely made up of fibres. This is rather a startling announcement for physiologists, since a great deal of present speculation is founded on the opposite opinion. Still more so is Dr. Stilling's statement, that these fibres hold an uninterrupted course from the *pons* to the *cauda equina*, without making any contributions to the roots of the nerves. In what way they terminate in the *cauda equina* he is unable to determine, but states that in their intermediate course they are nowhere seen to form anastomoses with one another.

The "spinal bodies" are minute corpuscles of irregular form, three, four, or five cornered, provided with a nucleus, and with long radiate processes, resembling on a larger scale those of the bone-corpuscles. Dr. Stilling is of opinion that, by means of these processes, they form a connected system, but cannot speak positively as to the point. Their exact relation to the fibres is not determined. The most important point in their history is, that they are only found among the fibres of the anterior grey matter. Here they are distributed with some regularity through the whole length of the cord, being much more abundant, however, at those points from which the nerves pass off to the extremities. From this, and other relations which they have with the anterior roots of the nerves, Dr. Stilling infers that their office is dynamic, and is connected with the motor power of the roots. For similar reasons he supposes that the *substantia gelatinosa* bears an analogous relation to the functions of the posterior roots.

The transverse fibres of the cord are, according to the author, nothing more than the immediate prolongations of the roots of the nerves. The course he assigns to these is very remarkable, since, if his accounts be true, the anterior and posterior roots are continuous. The afferent fibres which enter the cord on one side pass out as efferent fibres on the other, a great part of them crossing to the opposite half of the cord. Some of these pass in front, and others behind the spinal canal; and, in consequence, a double crossing and manifold intermingling of afferent and efferent

fibres take place,—the whole forming the anterior and posterior cross commissures. Under this view, the nerves may be regarded as great circles or loops, of which a very small segment is brought in contact with the longitudinal fibres proper to the cord.

The anatomy of the *medulla oblongata* is more complicated. The elements already described are interwoven in a more intricate manner, and new elements are added. The description of these last would be unintelligible without plates. The most interesting point in the account of this structure is the statement that the *vagus*, *glossopharyngeus*, and, in part, the *accessorius*, bear the same relation to the *hypoglossus*, as the posterior to the anterior roots of the spinal nerves. The remaining roots of the *accessorius* have a similar origin to that of the anterior spinal roots. It must be understood that here, as below, the roots of the nerves are continuous, and form in the same way the transverse commissures.

If we now review this brief summary of Dr. Stilling's observations, the facts that most strike us are those which concern the nature of the grey matter, the continuity of the anterior and posterior roots of the nerves, and, lastly, the relation of the fibres composing these, to the longitudinal fibres of the cord. Without adopting his anatomy of the two last-mentioned structures, which is, moreover, at variance in many essential points with Hannover's account of the same parts, I may remark that it derives support, as far as analogy can give it, from some recent researches by Mr. Newport, who has described exactly similar relations in the nervous system of the myriapoda.

The "spinal bodies" to which the author attaches so much importance will, probably, be considered by most anatomists as nothing more than a variety of the nucleated cells which are so abundant in most parts of the nervous system, of which grey matter is a constituent. For, although the greater number of such cells are round, yet many of them are furnished with processes exactly like those of the "spinal bodies," so that every gradation, in fact, may be found between the two. Not the less important, however, would be the fact, if true, of their being found exclusively among the fibres of the anterior grey tract.

I must remark that it rather damps our faith in Dr. Stilling's competence as a microscopic observer, to find that these bodies were described, in his first series of papers, as *dilated or varicose capillaries*.

But what casts still greater doubt over the results of his observations, is the fact of their having been made, exclusively, on specimens hardened by immersion in spirit. For, in spite of the statement that he has ascertained by comparison with fresh specimens, that the ultimate anatomical elements do not suffer much change from this treatment, the contrary is known to be the case,—at least, to a sufficient extent, to render microscopic examination confused and obscure. The increased hardness of the tissue, to obtain which the spirit is used, is the best of all proofs of a material chemical change.

It is to be hoped, however, that the evidence of observations, the greater part of which are at once so novel, and of such great physiological interest, will soon be examined anew by other anatomists, and under more varied modes of investigation.

Hannover's researches are, as far as the brain and spinal cord are concerned, of a more elementary kind. Like other observers, he finds that these structures are made up of two ultimate anatomical elements,—nucleated cells and fibres.

Cells are present in both wherever the substance of these structures is not white, but in the purely white matter not one can be found. They vary much in size in different parts, are generally round, often oval, sometimes triangular, or with one end drawn out into a point, while the other is rounded. He does not describe any differences between the cells of the brain and those of the spinal cord. In both, they are often seen, giving origin to fibres. He speaks very positively as to this fact; gives numerous drawings of it, and, indeed, seems to be of opinion that all the fibres of the brain, at least, originate from cells.

In the layer of white matter that has already been mentioned in another page, as forming the outermost layer of the convolutions, the fibres run parallel to the surface; but in the interior of the brain they pass from the convolutions to the base, and have, therefore, for the most part, a perpendicular direction. Numbers diverge into the cerebral nerves, while others proceed downwards, to form the cord. In direct contradiction to Dr. Stilling, Hannover states that the longitudinal fibres of the cord emerge in bundles to form the roots of the nerves, which are, in fact, according to him, continuations of them. In thus becoming nerve-fibres they offer no appreciable change of appearance.

Hannover also describes transverse fibres in the cord, but confesses that the great difficulty of the investigation prevented him from determining their true relations. He states that the peripheral terminations of the fibres of nerves are of several kinds. He agrees with other observers that in muscles they always end in loops, but denies that this is the case in the retina, or that they here form a plexus, as some have asserted. In his drawings of the retina the fibres are shown lying parallel to one another, and he gives it as his own opinion that they terminate in free ends. In the skin also, according to the author, they often end in the same way.

His description of nerve-fibres agrees pretty well with that of Remak, and he gives abundant proof that the central axis is a natural element of it.

The fibres of the sympathetic differ from the cerebro-spinal, in being much smaller and of more simple structure; in the absence of the central axis; and, also, in being garnished with nuclei along their whole course. He states, that in the ganglia they can be very distinctly seen originating from the true ganglionic cells—a point of some importance in relation to discussions now pending as to the independence of the ganglionic system.

Besides these, Hannover's work contains some observations on the nervous system of young and fœtal vertebrate animals, as also on that of a few invertebrata. The results offer no particular interest beyond that of confirming, in a general way, those already passed in review.

Lastly, must be mentioned his very accurate and excellent account of the retina, and of the expansion of the acoustic nerve. The anatomy of the retina, in particular, is admirably done, and appears to form the chief merit of the work: but as it would be doing this observer a great injustice to quote it apart from the beautiful plates by which it is accompanied, and without which it would be all but unintelligible, we may pass on to other matter.

It is pleasing to the mind to leave these regions of the vague and doubtful, for the more precise and purely experimental investigations of Dr. Marshall Hall. In this difficult and intricate part of anatomy there is good reason to believe that *function*, like optical phenomena for crystals, will for some

time yet, continue to be a more searching and intimate exponent of structure, than any to be found in our present methods of direct examination.

In the *new* memoir on the *Nervous System*, by the author just named, there is not much to justify the title. With a good deal of old matter in a new dress, there are to be found a few novel remarks and suggestions of great interest; while through the whole we have continual occasion to admire that clearness and originality of thought, and that matchless ingenuity, which communicate a never-failing interest to all the productions of this distinguished physiologist, and may well give us cause to pardon occasional repetitions.

The following remarks on the functions of the *medulla oblongata*, although not strictly original, have much interest at the present time:—

"I have mentioned that the cerebral system is the seat of the emotions, passions, and sensations. This part of the nervous system seems, indeed, to be the precise point where the *psychical* and *physical* functions are *combined;* the cerebrum being otherwise the exclusive organ of the former, the true spinal marrow of the latter. When the cerebrum and cerebellum are removed, and with these all perception, volition, &c., there is still the unequivocal expression of *pain*, when any part naturally endowed with great sensibility is forcibly seized with the forceps."*

After quoting the well-known experiments of M. Flourens, on this part of the nervous system, he adds:—

"The medulla oblongata seems, therefore, to be the central organ both of psychical and excito-motor phenomena, and to differ from the cerebrum in possessing the latter faculty, and from the spinal marrow in possessing the former. It is like certain nerves, *mixed* in its functions, and the common boundary of the cerebral and true spinal systems inclusive. It is probably by irritating the medulla oblongata that tumours in the median lobe of the cerebellum excite the sexual faculty and organs."†

But the chief interest of the memoir consists in the beautiful and complete demonstration it gives of the purely reflex character of the respiratory acts.

In none of the former productions of this, or any other

* *Op. cit.*, pp. 33, 34, 35. † *Op. cit.*, p. 51.

physiologist that I remember, was this demonstration so complete and satisfactory, that doubts on the point might not still be legitimately held, especially by those acquainted with M. Flourens' investigations. Dr. John Reid had, indeed, come very near it. But, with these new researches of Marshall Hall, this great truth is finally made a part of science. As its acquisition will, probably, in the end, be considered one of the greatest achievements of modern physiology, I shall transfer to these pages the author's account of the principal experiments on which the demonstration rests.

"If in a kitten, within the first nine or ten days after birth, we remove the cerebrum, divide the pneumo-gastric nerves, and open the trachea, the respirations gradually fall in number, until they become repeated but three or four times in a minute. If now, during the intervals between the respirations, we direct a stream of air forcibly upon the animal, or irritate the nostril, the anus, the tail, the foot, or, in a word, any part of the surface, or jar the table, an act of inspiration is immediately excited out of the usual course of these acts. This phenomenon is constant. It is the more marked, the younger the animal. In a kitten within ten days from birth, it continues for an hour or two. It is still observed in kittens of three, four, and five weeks old; but at a later period the experiment is apt to fail: much, too, depends on the degree of loss of blood which has been sustained during the removal of the cerebrum. In favourable circumstances, the slightest breath of air, the slightest motion, &c., induces an immediate inspiration.

"Such are the proofs of the influence of the tri-facial and spinal nerves in exciting acts of inspiration. The proof of the influence of the pneumo-gastric in exciting the same acts, is afforded by removing the cerebrum, and then dividing first one, and then the second of these nerves: the number of the acts of inspiration, *before* and *after* each of these steps in the experiments, gives us the measure of the influence of each organ, as they are successively removed. In one experiment the respirations were very frequent before the removal of the cerebrum; about forty after its removal; about ten after the division of one pneumo-gastric; and three after the division of the second. Acts of inspiration were still excited in the manner already described, by

directing a current of air upon the general surface, or applying any other form of irritation."*

It is, certainly, a very beautiful result to show that such an important function as that of breathing, depending for its exercise on rhythmic movements continued incessantly through life, is maintained by a *purely physical* principle of action. The fact, however, is not difficult to conceive, seeing what modern invention has succeeded in accomplishing, by combining a simple moving power with mechanism.

In the rhythmic movements of the steam-engine we may find a not unapt parallel to the phenomena of breathing, which they so well mimic,—a parallel in which, perhaps, there is more and deeper analogy than meets the sense in the outward circumstance,—and one which may well abash the boldness of those who deny the theory here given of these phenomena, on the ground of the insufficiency of the means to the end attained. If man, with his limited powers, can accomplish so much, what may not the Great Contriver of the universe perform by similar means.

From other parts of this memoir it appears, that Dr. Marshall Hall still adheres to the theory he has already given of the acts of coughing, sneezing, vomiting, &c.; and although he admits that the last-named of these acts is sometimes of *centric* origin, he seems to be of opinion, that it is far more commonly a reflex phenomenon. In illustration of this view, he introduces into his diagrams, nerves from the vagina, uterus, ureter, gall-duct, and other parts, through which the impulse is supposed to find its way to the common centre of reflex acts—the cord—thence to be reflected on the stomach.

Nowhere, however, is it explained, how phenomena that are brought about by a principle of action admitted to be purely physical, and for the direction of which on particular groups of muscles, in order to the performance of these special acts, a corresponding grouping of nerves more or less closely about a central point, would seem to be a necessary condition, can be

* *Op. cit.*, pp. 262–6. Again,—"The same phenomena are observed in cases of partial asphyxia, induced by retaining the animal for a certain time in a limited portion of atmospheric air. When removed and laid upon the table, acts of inspiration may be excited by any of the means I have mentioned. A current of air induces an act of inspiration only: irritants applied to the general surface induce common reflex movements, together with the act of inspiration."—p. 265.

caused alike by impressions on nerves which impinge on the common centre at such various and distant points. It is remarkable that this, among the many apparently insuperable difficulties in the way of this theory, should not have occurred to its ingenious author. Some misgivings of it, however, may perhaps be discovered in the following passage:—"It is remarkable that the medulla oblongata should be the central organ of the reflex acts in so *many* physiological arcs!"

I would venture to suggest, that in connecting this idea with that of the possession of *sensation* by this structure, we shall find the best key to the difficulty. But, probably, the author considers this difficulty already solved by the instance of the act of breathing; an act quite as special in character as any of the group, and which experiments already cited clearly prove may be excited (alike) by nerves arising from the most distant points of the surface, and which enter the spinal marrow at points as remote from each other, as those supposed to convey the exciting impulse in the other instances.

But, on close examination, the cases will be found to be essentially different. Breathing is a permanent function, incessantly necessary to life, and the mechanism for it is permanently laid down in a fixed anatomy: and while experiments show, conclusively, that it may be kept up as a purely reflex act, by impressions on nerves of very distant parts, the experiments of M. Flourens prove, on the other hand, that, to this end, the integrity of some such central grouping as that already surmised is necessary, since the slightest damage done to *one point* in the medulla oblongata, instantly and finally abolishes the act. Is there a corresponding and independent arrangement of the same kind for each of the other acts here considered—for vomiting, sneezing, coughing, &c.? Admitting the agency concerned to be purely physical, this would be absolutely necessary under the supposition; for otherwise, the only conceivable source of variation that could be introduced to account for the varying re-action in these several acts, would be the admission of the excito-motory impulse to the cord, in more than common force, through particular channels.

Something, perhaps, may be claimed for differences in the *mode* and *intensity* of the impressions communicated. But, although it must be admitted that variety in these conditions will, of necessity,

introduce some difference into the character of the resulting act, yet it can be clearly proved, by conclusive facts, that this is quite insufficient to account for the change in the whole course of innervation, and for that special direction of it into particular groups of muscles, while neighbouring muscles are thrown into complete relaxation, which characterise the acts here considered, and distinguish them from one another.

This is not the place to bring forward the evidence, by which I think it may be shown, that another and wholly different principle of action is here introduced, and is an initial and essential condition of the phenomena; in short, that the old theory, as Dr. Alison has all along held,—that these acts are governed by peculiar and appropriate sensations,—is the true one. It may, however, be remarked, that the act of laughter caused by tickling, alike, whether the *sensation* of tickling be excited through the nerves of the foot, or those of the sides, may be taken as an exponent of the true physiology of the whole group, and also as a clear illustration of the views here maintained.

It may also be added, that the prerogatives of sensation and emotion,—for the two blend in one principle,—in directing motor power into particular groups of muscles, with singular variety of result,—prerogatives, co-ordinate as they are in this respect with those of the *will*,—have never been sufficiently considered in their applications to physiology and pathology. It may be safely predicted, that much that is now, with a confidence that knows no doubt, very differently interpreted, will one day find its true explanation in these applications. In support of this, I need only point to the phenomena of hysteria and mesmerism, to be sufficiently understood, although the observation may easily be extended to other subjects.

I am sorry to be obliged to dismiss in few words Mr. Newport's "Anatomical and Experimental Researches on the Nervous System of the Myriapoda," published in the last number of the *Transactions of the Royal Society*. The gist of Mr. Newport's paper lies in the description of what he supposes to be a true reflex anatomy in these animals, in the shape of certain fibres, which, entering the nervous cord or axis from without, do not pass on to the analogue of the brain, but after a short and uninterrupted course,

again emerge, without any break or any junction with other fibres, to supply corresponding parts to those, from which they were traced inwards. Other fibres of the same class pass directly across the cord from one limb to its fellow on the other side. In their passage through the cord all these fibres lie imbedded in grey matter, which is made up of nucleated cells. It need scarcely be remarked, how forcibly these details recal to mind the very analogous relations described by Dr. Stilling in the spinal cord of man.

Besides those already described, there are two other principal elements in the central nervous axis of these animals,—a ganglionic and an aganglionic tract,—which, as they may be traced directly downwards from the analogue of the brain, Mr. Newport regards as ministering to sensation and volition.

Moreover, as, after the removal of the cerebral ganglion, the motions of these animals, although still active, cease to give evidence of volition and sensation, (the entire absence of the latter, however, not being always perfectly clear,) Mr. Newport considers himself entitled to infer that these motions are purely reflex, and are executed through the fibres that take the peculiar course already described.

It is highly desirable that these observations should be repeated by other anatomists, especially as Helmholtz* regards the analogous relations, formerly described by Mr. Newport in the nervous system of the crab, as, in great part, the artificial result of the mode of examination.

However this may be, the present memoir is full of interesting matter of various kinds, and will well repay an attentive study. The physiologist cannot, in particular, be too strongly recommended to the study of the motor powers of animals, in their involuntary exercise, under every variety of anatomical condition in connection with which they may be exhibited.

There is, indeed, already much to hope from the continued and active investigation of this branch of physiology by so many able inquirers, and under such various modes of investigation. For it is not difficult to see that among the so-called reflex phenomena of the spinal cord, and the equivalent phenomena of the

* I have been unable to obtain this author's work on the microscopic anatomy of the nervous system of invertebrata—*De Fabrica Systematis Nervosi Invertebratorum.*

invertebrata,—in short, among the *purely automatic* phenomena of the nervous system,—lies the great theatre of future discovery in the physiology of this system at large.

Besides the beautiful applications of the discoveries already made, in this department, in elucidation of various functions of vital importance, the discoveries themselves have another and yet wider interest, in revealing the nervous agency to us under conditions of manifestation, in which its laws of action may be made the subject of precise and direct investigation. For a long time physiologists were baffled and embarassed in this inquiry, by having to observe this agency more or less in complication, fancied or real, with sensation and volition. Having, for their contemplation in these latter endowments, powers that are interpreted by no analogy, and that have no standard of comparison beyond their own sphere of action, an element was thereby introduced, which enveloped the whole inquiry in impenetrable and hopeless obscurity. But this obscurity has been finally dispelled by the discovery, for the first time consummated by the admirable investigations of Dr. Marshall Hall, that the motor powers of the nervous system, exercised through the spinal cord, survive the entire extinction of the psychical powers, by the removal of the structures which minister to these last, and may thus be obtained for observation in the most complete isolation.

Few seem to have apprehended that of this discovery, this may one day prove to be the most important result. Hereby stripped of the complication in which it usually acts in the living animal, the nervous agency is brought under observation in its simplest mode of manifestation, under the form of an impulse, acting at considerable distances, and through large masses of structure, so as to offer great advantages for determining the laws of its propagation. So that here, in our investigation into one of the highest prerogatives of animal life, we unexpectedly meet with the facilities and conditions of common physical inquiry; having, moreover, for our subject, the properties of a power, acting in implicit submission to mere physical impressions, and, as it would seem, with a purely physical result; for the ultimate result of all these spinal acts, call it muscular contraction, or what we will, *is an exertion of motor power that may be measured in mechanical units.* However this may be, if we would discover the nature and

properties of this power, and its relations to other agents, the most philosophical course to take, and that which has been followed with greatest success in other branches of science, is to determine the laws of the phenomena effected by it. To whatever this may lead us, whether—as Dr. Holland puts the question—"to infer the identity of the nervous agency with any of the powers surrounding us in nature, or to admit that it has no type elsewhere in creation," the result must be, in either case, of vast importance.

To determine, therefore, *in their most general expression*, the laws of the phenomena now styled *reflex*, and to ascertain, in their simplest and ultimate form, the essential anatomical conditions of the manifestation of these phenomena, would, in itself, be a discovery of the highest order. Besides the vast addition that would thus accrue to our knowledge of the properties and nature of the nervous power, it is impossible to foresee what light it might not throw on the phenomena of sensation and will.

It is evident that these two principles of action, also, re-act on the world without by help of this same agency. The singular and varied adaptation of means to ends observed in numerous purely spinal and automatic phenomena, has already led physiologists to remark with what curious accuracy these phenomena "mimic the motion excited by the will and guided by the understanding."* Indeed, it was the very perfection of this mimicry that so long misled Legallois and other physiologists, and veiled from them the true character of the phenomena they observed.

And even in regard to other sensorial functions not resulting in muscular contraction, and which to us, in our present knowledge, seem of an *essentially* different character, in so far as these are the work of physical instruments, and are by that much subject to physical conditions, who can tell what elucidation may not come from this line of investigation? Relations of the most interesting kind may thus be unfolded, of which we can at present form no adequate or distinct conception. Of the possible extent of such elucidation, we may form the best estimate in considering the immense variety of phenomena, scarcely less different in outward character than are these varied actions of the nervous system, which, in optics and, still more, in electricity, the discovery of a

* See a very able article on "The Researches of Stilling, Hannover," &c., in the *British and Foreign Medical Review*, July, 1844.

single law, or the detection of a single agency, has brought under the interpretation of one common principle.

It must be remembered, too, that these actions of the nervous system are exercised through parts which are of common aspect, and consolidated in structure and functions; so that in discovering the essential anatomical conditions of the automatic phenomena, and in ascertaining the true relations of the ultimate elements of this anatomy, we may best hope for such interpretation as may be within our reach, of the relations of the more complicated structures which minister to the higher functions.

In the investigation of this department of the physiology of the nervous centres, pursued with these views, we have as yet only broken ground; for the admirable discoveries of Marshall Hall, important as they are, and well deserving to immortalise his name, carry us but small way towards these objects. Perchance their author might consider such objects altogether unattainable, or the pursuit of them, through this channel, a retrogade movement. And, undoubtedly, the difficulty, so well stated by Dr. Holland, does occur here, namely, "Whether we can reason upon a single agent, where the functions differ thus widely in their nature?" "What we may justly infer, however,"—to use the words of the same writer,—"looking to the common aspect of nervous structure, and to the connection of nerves of different classes with each other and with common centres, is its being an element of the same nature, capable of similar relations of quantity or intensity, of translation to different organs along the course of nerves, and of suspension by like causes of injury or disturbance."

If this be so, who can estimate too highly the privilege with which late investigations have endowed us, in presenting this agency to our observation under a form of manifestation so open to precise investigation; or who can judge of the degree of light which the discovery of its laws of action by these means, may probably throw on the phenomena of sensation and will. But if this discovery may justly be expected to be of the highest importance in a purely physiological sense, still more instructive must its other relations be in their bearing on the nature of the mechanical powers with which animals are gifted. For although it has been shown, by direct experiment, that spinal innervation is not the source, or, at any rate, not the only source of the contractile

power of muscle, yet the astonishing effect of strychnia in increasing this, sufficiently shows an intimate and essential relation between the two.*

The nature and source of these powers in their joint exercise have long been a subject fruitful of speculation. Hitherto, however, the field has been almost wholly left to the metaphysician and the natural philosopher. Among physiologists, only the more speculative have ventured to break ground in it. The more practical men have always held aloof, either regarding such inquiries as a waste of time, or looking upon all hopes of their leading to any material discovery, as vain and delusive. Of late, however, clearer ideas have begun to prevail on the subject, and most physiologists of the present day see, with more or less distinctness, that the motor force which animals have at their command must be a purely physical power, and that its relation to other physical agents is not only a legitimate object of investigation, but one that almost certainly lies within the limits of discovery. Few, however, even now suspect the deep interest such a discovery would necessarily have for the practice of medicine, and its intimate bearing on every part of the same. Yet this strikingly appears from the large part which the motor powers of animals fill in the business of life, from the need of their incessant exercise for its maintenance in higher animals, through the acts of breathing and circulation; from the frequency of the sudden extinction of these powers by various deadly agencies; from their susceptibility of more or less rapid exhaustion; and the need of rest and *new material* for their repair.

Recently this, along with almost every other high subject in physiology, has been dealt with by the chemists; and it must be confessed that they have handled it with great boldness, and have apprehended its true relations, as an object of inquiry, with great clearness of thought. To them we are mainly indebted for the more distinct ideas upon it, which are beginning to make their way among physiologists; and in connecting with one another, by material and quantitative relations, the two grand characteristics of animal life, namely, the combustion of matter and the production of mechanical force, they have probably thrown out an idea

* I need scarcely remark, that the direct action of strychnia is confined to the cord, and only reaches the muscle through the nerve. This admits of experimental proof.

of deeper significance than any hitherto developed by those who have only looked at the subject from other points of view.

But the defect of the chemists has been, in confining themselves to the consideration of this force in complete abstraction from organisation and structure, so that when we come to apply their views to the actual physiology of the structures through which this power is exerted, we find ourselves as far back as ever. This gap the physiologist alone can fill up, and it is to his investigation of the laws of spinal or automatic nervous action that we must look as the most likely path to discovery. Here, then, he may find objects of sufficient scope and elevation to satisfy the cravings of the most lofty ambition. But he cannot expect to attain results commensurate with the most elementary of the objects here proposed, unless by keeping these objects in view, clear and well defined, and by following them out by a large scheme of investigation. Nothing less will avail than the systematic study of the agency which we have been considering, under all the various anatomical conditions in which it may be observed through the animal kingdom, or to which it may be reduced by experiment. It is only thus that we can hope to arrive at the essential conditions of its manifestation, and through these to gain some insight into its relation with other powers in nature, whose laws are better known. A scheme of such scope and purpose has never yet been acted upon.

In the anatomical part of the investigation, more systematic reference than is apparent in any of the works passed in review, should be made to embryology, where adult anatomy is so intricate and involved. It was an ingenious and fertile thought, to unravel the mazes of finished texture by observing the tissue in its laying down; to watch the shuttle of the weaver and mark the pattern of his woof.

More, still, as indeed may already be gathered from Mr. Newport's instructive investigations, is to be expected from comparative anatomy, with its yet more simple analysis of structure, and with all its chances of pregnant instances. Already one such—and, as it appears to me, one of no mean import—has come to light from this quarter, in the shape of a small fish, named the lancelet. (*Amphioxus lanceolatus.*) The anatomy of the nervous system of this fish, of which the absence of all outward distinction between

the brain and spinal cord is, in itself, a very singular feature, is so remarkable throughout, that I shall quote, entire, Mr. Goodsir's account of it as given in the last number of Dr. Todd's *Cyclopædia*, referring the reader for further details to the original paper by the former, by whom it was first accurately described:*—

"When a portion of the spinal cord is examined under a sufficient magnifying power, it is seen to be composed entirely of nucleated cells, very loosely attached to one another, but inclosed in an excessively delicate covering of pia mater. The cells are not arranged in any definite direction, except in the middle third of the cord, where they assume a longitudinal linear direction, but without altering their spherical form. The black pigment formerly mentioned as existing, more particularly on the upper surface and groove, is observed to be more abundant opposite the origin of the nerves; and as it is regularly arranged in this manner in dark masses along the anterior and posterior thirds of the cord, the organ, in these places, resembles much the abdominal ganglionic cord of an annulose animal. Along the middle third the pigment is not so regular, but appears in spots at short intervals. When any portion of the cord, however, is slightly compressed, and microscopically examined, it becomes evident that there is, along the groove and mesial line of its upper surface, a band consisting of cells of a larger size than those composing the rest of the organ. Some of these cells, only, are filled with black pigment, but all of them contain a fluid of a brown tint, which renders the tract of large cells distinctly visible. When the compression is increased the cells burst, and the fluid which flows from the central tract is seen to contain jet black granules, which may be detected as they escape from the cells. The nerves consist of primitive fibres, of a cylindrical shape, with faint longitudinal striæ.

"The primitive fibres of a trunk pass off into a branch, in the usual way, without dividing; and where the trunks join the spinal cord, the primitive fibres are seen to approach close to it, *but without passing into it.* The greater part of the slightly protuberant origin consisted of the nucleated cells of the cord, with a few pigment cells interspersed; the exact mode of termination of the central extremities of the primitive nervous fibres could not be detected.

* *Transactions of the Royal Society of Edinburgh*, vol. xv., Part 1.

"One of the most remarkable peculiarities in the lancelet is the absence of the brain. Retzius, indeed, describes the spinal marrow as terminating considerably behind the anterior extremity of the chorda dorsalis, in a brain which exhibits scarcely any dilatation; but careful examination of the dissection of my own specimen, which I have also submitted to the inspection of Dr. J. Reid, and of other competent judges, has convinced me that the spinal cord, which may be traced with the greatest ease to within $\frac{1}{10}$th of an inch of the extremity of the chorda dorsalis, does not dilate into a brain at all. It may be urged that we ought to consider the anterior half of the middle third of the spinal marrow, where it is most developed, to be the brain, and all that portion of the chorda dorsalis which is in connection with the branchial cavity, as the cranium. That this does not express the true relation of the parts, is evident from the fact, that this portion of the cord, to its very extremity, gives off nerves which are too numerous to be considered as cerebral, but more especially from the mode of distribution of the first and second pairs, which in my opinion, proves the anterior pointed extremity to be the representative of the brain of the more highly developed vertebrata. A brain of such simplicity necessarily precludes, on anatomical grounds alone, the existence of organs of vision and hearing. These special organs, which are developed in the vertebrata, at least, in a direct relation with the cephalic integuments and the brain, could not exist, even in the form of appreciable germs, in the lancelet.

"The peculiarities in the structure of the spinal cord are not less remarkable than those of its configurations. It is difficult to understand, according to the received opinions on the subject, how a spinal cord destitute of primitive fibres and tubes, and composed altogether of isolated cells arranged in a linear direction, only, towards the middle of the cord, can transmit influences in any given direction; and, more especially, how the tract of black or grey matter, if it exercises any peculiar (excito-motory?) function, communicates with the origin of the nerves. The nerves also are remarkable, originating in single roots, and containing in their composition only one kind (cylindrical) of primitive fibres."

There can be no doubt that as our knowledge of this subject enlarges, and our inquiries begin to shape themselves out into definite questions, we shall find in this singular creature, in which

the functions of the nervous system are reduced to such simple anatomical conditions, much elucidation of what is obscure and complicated in vertebrata of higher organisation. It is not difficult to see the close and important bearing which the anatomy of its spinal cord already has on the present course of speculation, on the subject of the so-called reflex phenomena.

I shall bring this part of my subject to a close with a brief notice of the observations, by which Bidder and Volkmann have lately sought to prove, on anatomical grounds, that the *sympathetic* is not, as some have taught, an offset from the brain and spinal marrow, but must be regarded as an independent and separate system. The evidence on which they lay most stress, and which they seem to consider as in itself sufficient to establish the point, is set forth in the following statements:—

1. The fibres of the sympathetic are much smaller than those of the cerebro-spinal, insomuch that the largest fibres of the former are separated from the smallest of the latter kind, by a broad gap, which plainly divides them into two distinct groups.

2. That in the frog, all the sympathetic fibres to be found in the roots of the cerebro-spinal nerves, would not, if added together, make one-third of the whole amount of such fibres contained in the peripheral branches of the sympathetic system.

3. That a very large proportion of the fibres of the sympathetic are derived from its own ganglia, in which they may be seen plainly originating from the well-known corpuscles which form the granular matter of these bodies. An observation to this effect has already been quoted from Hannover.

In further support of this statement it is asserted, that in the ophthalmic ganglion of the cat, the efferent fibres are four times, and in the cœliac ganglion of the same animal, three times as numerous as the afferent.

In addition to these more important facts, the authors bring forward certain anatomical characters, which serve to distinguish the two classes of fibres from one another. As these have already been quoted in nearly the same terms from Hannover, they need not be repeated here.

In answer to these statements, Valentin, who is the great champion of the doctrine that the sympathetic is a mere offset of the cerebro-spinal system, makes the following objections:—

1. That although the fibres of the sympathetic be, on the whole, smaller than those of the cerebro-spinal, yet there is a gradual transition from one series to the other. That no such gap as that mentioned by Bidder and Volkmann, as dividing these fibres into two distinct groups, really exists, since the space it fills in linear measurement is so small, as to fall within the common and necessary errors of observation. That, moreover, the fibres of all nerves vary much in size, and that many single fibres do so, in different parts of their course, so that the fact does not justify any physiological inference.

2. That the fibres which originate in the ganglia are not true nerve-fibres, but the pseudo-nerve-fibres described by Remak, and that these last make up the whole excess of efferent over afferent fibres.

According to Valentin's description, these pseudo-nerve-fibres are merely empty sheaths, thrown off from the ganglionic corpuscles, of the same kind as the exactly similar sheaths, which this anatomist describes as passing off from the same corpuscles to invest the true nerve-fibres.

These objections have drawn a rejoinder from Volkmann in defence of the original paper, and a very acrimonious discussion is now being carried on between these anatomists, into which it would be improper to enter here. It may be remarked, however, that this appears to be a question which cannot be finally settled on purely anatomical evidence; for the only absolute proof, on whichever side it may be, must rest on dynamic, or physiological grounds.

Terminating here this rapid sketch of the recent progress of the anatomy and physiology of the nervous system, we may pass at once to the electric phenomena of animals.*

* I regret being obliged to pass over a great number of original communications on points of minor importance. For the convenience of those who may desire further information, I subjoin a list of references to a few of the more interesting among them:—On "The Weight of the Brain and Cerebellum," see a paper by Dr. J. Reid, *Edinburgh Monthly Journal of Medical Science*, April, 1843; also in *Paget's Report*.—On "The Direct Influence of the Nervous Centres," by Volkmann, *Paget's Report*, and *Müller's Archiv*, 1842, heft. v.—On "The Structure of Ganglia and Origins of Nerves in Invertebrata," by Dr. F. Will, *Müller's Archiv*, 1844, heft. i. and ii.—*Observations on the Nervous System generally*, by Heine and Klencke. (An abstract of this work is given in *Valentin's Report*.)—*Physiologie des*

That such a transition should not be felt to be abrupt, but, on the contrary, should be almost expected, bespeaks, perhaps, better than anything that could be said, the high interest of the subject. A very elaborate work upon it has recently been published by M. Matteuci, whose numerous detached observations on these phenomena, which have, from time to time, appeared in the *Reports of the French Academy*, are, probably, already known to the members of this Association. The present work contains these observations in a collected form, together with many new experiments, and the author's matured views on the theory of the phenomena.

The first part of the work is taken up with proving:—First; the existence of electric currents in all *living* muscles in their passive state. Secondly; that of a current of constant course and direction peculiar to the body of the frog. And, lastly; the occurrence of an electric discharge in all muscles at the moment of contraction. All these points are well made out, clear from all fallacy arising from chemical change, difference of temperature, or other sources of electric disturbance at the point of contact between the apparatus and the subject of experiment.

The dependence of the phenomena on the living state, and their relation to certain organic conditions, are also clearly established. But the difficulty runs through them all, as to how far they have any essential *physiological* connection with the function of the living part, or be not rather mere epiphenomena of secondary importance.

Nevertheless, all these interesting relations are well worthy of being kept in view, in the present state of speculation in this part of physiology.

In regard to the exact source and nature of the phenomenon which occurs in muscles at the moment of contraction, the

Rückenmarkes, by Schultz. (This work contains an account of "Purkinje's "Observations on the Dura Mater of the Spinal Cord.")—*On the Physiology of the Nerves and Spinal Cord*, a Monograph, by W. F. Arnold.—On "The Nervus Accessorius," by Morganti, *Schmidt's Jahrbücher*, No. vi. 1844.—For Observations on the same Nerve, by Vrolik and Spence, see *Paget's Report*.—"Recherches Experimentales sur les fonctions du Nerf Spinal," by Cl. Bernard, *Archives Generales de Medicine*, Avril et Mars, 1844.—"Revision of some of the Doctrines contained in former contributions to the Physiology of the Eye," by Volkmann, *Schmidt's Jahrbucher*, No. v., 1844.

author expresses himself with great reserve and caution; and, in particular, he confesses himself unable to determine what relation, if any, it bears to the electric current, which may be discovered in all muscles in their passive state. But M. Becquerel, who has repeated M. Matteuci's experiment with him, asserts that the fact cannot be explained without admitting the occurrence of an electric discharge in the muscle; and seems to be confident in the opinion, that this discharge must have an essential and physiological connection with the act of contraction.

The experiment itself is very simple; and for the information of those who may wish to repeat it, I will here copy the author's own account of it, which is given in these words:—

"I place a frog, prepared in the ordinary manner, as in the original experiment of Galvani, on an isolated plane covered with oil-cloth. I then prepare another frog, so as to retain only one of its legs, with the bundle of nerves, from the spinal cord to the muscles of the leg, left hanging from it. To avoid error, it is necessary to have all the muscles of the thigh removed, and the nerves laid bare, and clear of other tissue. I then lay this filament of nerves on the thighs of the first frog, in such a way that the leg itself does not touch the thighs, and that the nervous filament connecting the two lies loose and free from traction. I then wait until the convulsive movements caused by the rapid preparation of the frog have entirely ceased. The experiment might indeed be made without this precaution, seeing the striking difference between these convulsive movements and the phenomena about to be described. If we now touch with a galvanic couple, the lumbar nerves of the first frog, the muscles of its thighs immediately contract, *and at the same instant the leg whose nerves lie across these thighs is suddenly convulsed.*"

When very lively frogs are used, the experiment succeeds equally well, on merely irritating the lumbar nerves of the first frog, without the galvanic couple. Numerous other experiments are related, which prove that when the galvanic couple is used, the convulsion in the second leg is altogether independent of any direct influence from it. M. Matteuci has also made the experiment, with the same success, in laying the nerves of a prepared frog's leg on the bare muscles of a living rabbit.

Whatever interpretation this phenomenon may ultimately receive,

the fact is rendered doubly interesting by the discovery, recently made by M. de Quatrefages, that in certain transparent *annelida* every act of muscular contraction is attended *by a flash of light;* this flash being, in fact, the cause of the phosphorescence of the animals in question.*

The current which M. Matteuci describes as being peculiar to the body of the frog, he very satisfactorily shows to have been the source of the phenomenon originally discovered by Galvani, and well known as the first germ of the vast science which still bears his name. Of this current, all that seems needful to be said here is, that it passes through the whole mass of the animal, and is quite independent of the nervous system. In the prepared frog, in fact, it keeps its direction and full intensity, though the animal be stripped of spinal marrow, of the spinal and crural nerves, and of every visible nervous filament in the muscular mass of the thigh.

In his inquiry into the conditions which affect the manifestation and intensity of the proper current of the frog, the author found them identical with those which affect, in the same way, the muscular current already spoken of, as common to the muscles of all animals. What adds to, or takes from, the intensity of one of these currents, has the like effect on the other. Whence the inference is pretty plain, that these phenomena probably have their source, at bottom, in the same molecular changes. Nevertheless, many remarkable points of difference are observed between them, not the least of which is the fact, that whereas the muscular current is found alike in the muscles of all animals, the "*proper current*" can be detected in the body of the frog only.† In salamanders, eels,

* See several numbers of the *Annales des Sciences Naturelles*, for 1843, and also the *Comptes Rendus de l'Académie des Sciences*, for January 15, 1844, which contains a Report, by Milne Edwards, on M. de Quatrefages' researches. In this Report Milne Edwards states, that in examining very attentively certain "Beroés" of the Mediterranean, which shed a vivid light around them, he had already remarked that the light proceeds from the ciliated ribs with which the bodies of these zoophytes are garnished,—these being the very parts in which the organs of motion are seated. I regret that it does not fall within the scope and objects of this Report to notice, at length, the admirable series of researches by M. de Quatrefages, which are so rich in observations of general physiological interest.

† One effect of this current is, that whenever the circuit is closed by connecting the upper parts of the animal with the muscles of the lower extremities, a convulsion ensues. This observation acquires collateral importance, from the fact that this cause of muscular contraction has been entirely overlooked by those physiologists

turtles, and other animals among the nearest akin to frogs, it was sought for in vain.

In another part of the work there is a very important investigagation into the phenomena that occur on making and breaking contact, when weak galvanic currents are made to pass through the nerves of living animals, or those of the prepared frog. I have not space to enter into the details of these experiments, but the most important result of them, stated in general terms, was this;—that after the first lively susceptibility has passed away, the passage of the current through the nerve in one direction, excites the muscles to contraction, while, in the opposite direction, it is without any effect of this kind. It is plain that this may one day become a fact of great importance, since it seems to imply a *dynamic* relation between the two agencies—galvanism and nervous force.*

But by far the most interesting part of M. Matteuci's work is that which relates to the phenomena of electric fish. This arises, no doubt, in part, from the inherent and undying interest of the subject. In the single instance in all nature, which these creatures afford, of a common physical agent placed under implicit submission to the *will* of a living animal—an agent, whose artificial development is the result of familiar and every-day experiment—there is, surely, something that must arrest the attention of the dullest witness of the fact.

In the possession of voluntary power over muscular contraction, we have, no doubt, *a fact of the same order;* but, here, the true nature of the phenomenon is obscured to us through our familiarity with it, and through our involuntary habit of con-

who have occupied themselves with investigations into the functions of the spinal marrow, and with whom, for obvious reasons, the frog has always been a favourite subject of experiment. It appears not at all improbable that some, at least, of the phenomena observed in these investigations, and set down to spinal agency, were, in fact, electrical. There is the more reason for supposing this, because M. Matteuci states that the electric phenomena occur, even when the circuit has to be completed through the body of the observer and the ground. We must infer, at least, from these facts, that no convulsions in the frog can be admitted as spinal acts, unless it be shown that they cease on destruction of the cord, or continue when the animal experimented on is placed on an isolated support.

* Through the kind courtesy of the author I was enabled to witness the whelo series of highly interesting experiments on which the statements in the text are founded, as they were performed by him in King's College, London, on his return from York.

templating it as a vital endowment, common to the whole animal kingdom indeed, but peculiar to it. But in the electric fish we have a case, in which the purely physical nature of the agent declares itself to sense, and has been made the object of experimental proof. Surely this is an instance of contact between the immaterial and the material—of submission of physical agency to psychical power, wonderful to contemplate.

May it not be viewed, without profaneness, as a symbol, humble, indeed, but distinct and, for that reason, valuable, of that entire submission to an INFINITE WILL, which reason and revelation alike teach us, is the condition of the powers of the universe.

The chief physiological interest of the subject consists, precisely, in the limitation of this extraordinary power to so few creatures. Here, for the first time in the animal kingdom, the physiologist meets with an anomaly, where no true anomalies are known; with the introduction of a new element, where all elements are in common; with a break in a chain whose every link is perfect. Without further inquiry, his mind revolts at the conclusion, and refuses the fact. And presently, and more and more as he dwells upon it, the conviction becomes pressing, that this must be only some other mode of manifestation of a power possessed by animals in common, and of whose nature it may thus become the exponent. And he is not slow to mark the numerous and deep-seated analogies which point to such a power in muscular contraction. In both, we find the same contact with the will, established by nerves, and subject to interruption by injury to, or division of, these; and, what is of deeper significance still, by nerves from the self-same anatomical tract. In both, the function *is exalted by strychnia and like agents*, and depressed by agents of opposite effect. In both, the exercise of function is followed by the same general exhaustion of nervous power, and, to each function, of its own force, requiring for the joint exercise of these *rest and a supply of food;* and, lastly, to the maintenance and full exercise of both, free respiration is, alike, essential. Surely, these numerous and deeply significant relations bespeak something more than a superficial analogy.

In particular, who can reflect on the intimate and perfectly equivalent relations of these two agencies to the nervous system, and fail to see that we are here upon the verge of discovery.

There is no part of M. Matteuci's work more interesting, than that which illustrates, in various ways, the entire dependence of the electric force of the torpedo on the nervous system, and the *dynamic* nature of the relations by which the two are connected. "I have observed," he says, "that when two or three of the nerves of the electric organ are divided, the discharge is afterwards limited to that part of the organ whose nerves are left entire."

Again, when all the nerves are divided, and the electric function is thereby destroyed, a discharge may be obtained by pulling with a forceps any one of the nerves, by the end in connection with the electric apparatus, just as a muscle may be made to contract, under the same conditions. But what is still more worthy of remark, is, that in this case the electric discharge is not general, but is strictly and accurately limited to that part of the organ which is supplied by the irritated nerve.

All these relations are illustrated, in a still more striking way, by the action of strychnia, which causes, on the one hand, a tetanic state of the muscles of the fish, and, on the other, and at the same moment, a rapid succession of electric shocks, *occurring of their own accord.* This is a fact that almost realises our idea of "a glaring instance." The direct and dynamic action of the drug, be it remembered, is *single,* and confined to the nervous centres. Only by its influence on these, does it affect the electric organ and the muscle. How clear, then, that its effects on these last are exact equivalents, and how striking the evidence of the intimate relation of both to the nervous force. Who can contemplate this circle of relations, without being reminded of the strictly analogous relations between electricity and magnetism, and other forms of the same power, which, to the minds of many, fore-shadowed the fundamental identity of these forms, long before this was finally established by experimental proof.

It has already been shown that, for the muscular force, and the electric force of electric fish, the conditions of action are identical, down to the minutest particulars. If this be true, of the conditions of their exaltation, it is not the less so, of those of their exhaustion, and of the resources needful for their repair.

"The Indians assured us," says Humboldt, in his *Personal Narrative,* "that when horses are put into a pond crowded with electric eels, not a single horse is killed the second day. These

fish require *rest and abundant nourishment*, in order to produce or accumulate a large quantity of electricity."*

M. Matteuci appears to have felt the force of the various relations sought to be unfolded in the preceding pages; and they are presented in a condensed form in his book, in a table drawn up by M. de Blainville. The work, or rather M. Matteuci's part of it, closes with a philosophical discussion on the relations of electricity to the nervous force. In this discussion, so far from showing any undue bias to physical views,—which we might naturally have expected from the nature of his pursuits, and for which this might well excuse him,—he goes so far as to declare, that not only can no evidence of an electric current be detected in nerves, but that with a knowledge of the properties at present assigned to electricity, and of its established laws of propagation, it is impossible to conceive such a current to be confined within the limits of a nerve.

Having stated thus much, however, he adds, as the final result of all his meditations on the subject, that there does exist between electricity and the unknown force of the nervous system, an analogy, which, if not of the same degree of evidence, is never-

* All facts that put broadly and prominently forward the great and fundamental relation between the consumption of material, and the various forms of force, which animals have at their command, are of uncommon interest in the present state of speculation. The passage quoted in the text always struck me as being peculiarly interesting in that point of view. The following principles, laid down with such admirable clearness by Faraday, in his discussion of the *contact* theory of galvanic force, apply with the same strictness to these relations:—

"The contact theory," he says, "assumes, in fact, that a force which is able to overcome powerful resistance, as, for instance, that of the conductors, good or bad, through which the current passes, and that, again, of the electrolytic action where bodies are decomposed by it, can arise out of nothing. *That, without any change in the acting matter, or the consumption of any generating force*, a current can be produced, which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others, by Oërsted and myself, show the convertibility of electricity and magnetism. But in no cases, not even those of the gymnotus and torpedo, is there a pure creation of force, a production of power, without a corresponding exhaustion of something to supply it."

theless of the same kind, as that which exists between heat, light, and electricity; a conclusion, we may remark, to which many other philosophers have come, and to which the present course of inquiry in all these subjects naturally tends.*

To complete my task, little more remains beyond a brief notice of the contributions made, during the past year, to the physiology of reproduction.

There must be few who have not been made aware, through the public prints, of the recent revival, with a feeling of very general interest, of the old theory of the analogy between the phenomena of *rutting* and menstruation, and of the essential relation of both, to the ripening and shedding of ova.

For the first promulgation of this theory in a complete form, we are, it seems, indebted to our countryman, Dr. Power. Unworthy attempts have been made, in various quarters, to rob him of his just merit in this matter. Although he brought forward no direct and undeniable proof of his theory, there can be little doubt that it was founded on a large induction, from analogy of various kinds; and it is not to be wondered at that he was satisfied with this, in a case in which all must admit that the force of analogy is unusually great.

Since the first publication of his views, Dr. R. Lee, Dr. Martin Barry, Dr. Paterson, M. Gendrin, M. Negrier, M. Pouchet, and others, have made investigations, which have contributed in no small degree to their final establishment. This seems to be now completely accomplished by the recent labours of Bischoff and Rokitansky. The former in discovering, at the period of *rutting* or *heat*, in the Fallopian tubes of various animals to which the male had never had access, the ova that had evidently just been cast off from the ovary, and the latter in proving, by an immense mass of evidence, the complete analogy between this function and that of menstruation, have furnished the only links that were wanting in the whole chain of evidence. It would be unjust, however, to the labours of former observers, not to state, that they had left but little room for doubt as to the last mentioned point.†

* I rejoice to learn that the Royal Society have awarded the Copley medal to M. Matteuci, for these most interesting researches.

† See, in particular, Pouchet's work, entitled *Theorie Positive de la Fecondation des Mammifères, basée sur l'Observation de toute la Serie Animale.* Paris, 1842.

The two works under review, and especially that of Bischoff,* are full of collateral matter of much interest. The impregnated ova which he found in the Fallopian tubes, had not acquired new envelopes, but were all naked, and in none was there any intimation of that peculiar division of the yolk, which marks the first stage of development of the impregnated ovum. He again brings forward numerous observations to prove, in opposition to Dr. Martin Barry, that the *vitelline membrane* of this physiologist has no real existence.

As an infallible sign that the ovum is becoming ripe and ready for discharge, he mentions the drawing out of the cells of the *discus proligerus* into forms approaching that of fibres; and, lastly, he states, that in animals, at least, there is no difference whatever between the *corpus luteum* of an unimpregnated, and that of an impregnated ovum.

With this account of the recent progress of science in relation to the preparations for the development of the new being, I might, not inappropriately, close this Report. But the physiologist is no longer confined to the consideration of the phenomena that occur within the narrow limits of living bodies. With the recent discoveries in chemistry he has extended the field of his speculation into realms that have the globe, with its atmosphere, for their narrowest boundary, and has recognised in one of the most universal powers of creation,—namely light,—the first cause of all organisation.

This sketch would, therefore, be very imperfect, were I to pass over without notice the speculations of the past year, touching that great interchange of matter ever going forward, in which the plant, by help of light, redeems back into organisation and life, for the future use of the animal, the matter which the latter is constantly reducing into dead chemical forms, and casting loose into air and earth.

Of these speculations, those of Professor Liebig on the supposed gradual defertilisation of the soil of England, claim our first attention, as they concern us all so nearly. His argument may be thus briefly stated:—

* *Beweis der von der Begattung unabhängigen periodischen Reifung und Loslösung der Eier der Säugethiere und des Menschen.*

The tissues of all animals are, it is well known, ultimately derived from plants. Plants must, therefore, contain, as essential constituents, the inorganic elements, as they are called, which are necessary to animal bodies. These elements are, again, derived from the soil; but analysis has shown that, here, their supply is limited. To maintain the fertility of the soil, therefore, and restore the balance, need must be, that the animal returns to the earth the whole amount of these elements which the plant has taken from it. The elements thus limited in their supply are, it is scarcely necessary to remark, the phosphates.

Now, Professor Liebig argues, and the fact cannot be gainsaid, that in effect of their domestic arrangements, the English annually waste into the sea a vast amount of these phosphates. In this he foresees, unless remedy be applied, the gradual defertilisation of our land, already to be discerned, he says, in the barrenness of numerous large tracts, that were once known to reward the toil of the husbandman with an abundant harvest. Of course, these remarks do not apply to the great extent of waste land, which has never yet been brought into sufficient cultivation to yield food for cattle. In these, he admits, we still have a large source in reserve.

In regard to these views, the first thing that strikes one is, that they seem very like an impeachment of the wisdom of Providence. That the fertility of a whole country, and the welfare of a vast people, should be made contingent on the practices which this people might adopt to ensure cleanliness, does not look like that self-acting compensation—those beneficent provisions for the maintenance of life, which our investigations have, hitherto, everywhere disclosed to us in the great scheme of animated nature. And although arguments from final causes are unsafe guides, and are inadmissible as substitutes for direct evidence, in dealing with scientific questions, they at least require that the evidence of conclusions that stand in striking opposition to them, should be well sifted.

In the present case, it will be found, on further examination, that Liebig has over-rated our danger, and, in particular, that he has not taken into account the various channels through which the ocean is made to give back her tribute. Nothing is said of the enormous weight of fish eaten every year by the inhabitants

of these islands, a great part of which, at least, leave their inorganic remains with us.*

Again, is it a work of accident, that six hundred British ships laden with guano—with phosphates, in short—are at this moment bending their sails to our ports; or, rather, may we not discern, here, one of the thousand ways by which, in the great scheme of Providence, this wonderful balance is ever being restored. In this great economy, in fact, loss is impossible. We may cast our phosphates into the sea, but there will be no waste. In the next moment, the floating and errant particle is redeemed into life by the plant or animalcule; these, again, are made the food of fish, and, through these last, the ocean is made to disgorge her prey to the inhabitant of the land. And thus is closed up and made perfect, that great cycle of life, of which the law is, never-ceasing change without loss.

The recent investigations of Professor Edward Forbes, touching the distribution of living beings in the depths of the ocean, have a very curious bearing on these speculations. The doctrine already incidentally alluded to, that plants, alone, have the power to organise simple chemical bodies, and that the tissues of all animals ultimately come out of plants, is well known, as the great deduction of modern chemistry. It is therefore rather startling to find the researches of Professor Forbes giving, at first sight, a direct and practical denial of it. The general result of his observations, first in the British seas, and more lately in the Ægean, was "to define a series of zones, or regions, in depth, and to ascertain *specifically* the animal and vegetable inhabitants of each. Regarding the tract between tide-marks as one region, which may be termed the *littoral zone*, we find a series of equivalent regions succeeding it in depth. In the British seas, the littoral zone is succeeded by the region of *laminariæ*, filled by forests of broad-leaved fuci, among which live some of the most brilliantly-coloured and elegant inhabitants of the ocean. This is the chosen habitat of Lacunæ, of Rissoæ, and of nudibranchous mollusca. A belt,

* It may seem a strange question, but it is one that admits of being asked,—Whether, if these phosphates were cut off from the sea, we might not shorten the breed of those fish that frequent our shores? Certainly, at least, we should diminish the supply of the mineral constituents of the microscopic beings which pullulate in such myriads in our deltas; beings, in which fish find one of their chief sources of nourishment, and which form one of their chief attractions to our coasts.

generally of mud or gravel, in which numerous bivalve mollusca live, intervenes between the laminarian zone, (in which the flora of the sea appears to have its maximum,) and the region of corallines, which, ranging from a depth of from twenty to forty fathoms, abounds in beautiful flexible zoophytes, and in numerous species of crustacea and mollusca, to be procured only by means of the dredge. The great banks of monomyarious mollusca, which occur in many districts of the northern sea, are, for the most part, included in this region, and afford the geologist his richest treasures. Deeper still is a region, as yet but little explored, from which we draw up the more massy corals found on our shores, accompanied by shell-fish of the class *brachiopoda*. Vegetables become fewer and fewer as we descend, and, at the end of one hundred fathoms, are reduced to a single species,—a *nullipora*. A small space below this, and no plant is found by the present means of search." But, strange to relate, through a vast region below,—a region which, in the Mediterranean, exceeds all the others together,—Professor Forbes finds the depths still inhabited by animals. These are, indeed, few in number; but even at the depth of 230 fathoms, as many as eight species of testacea are found. If it be true that animals ultimately derive all their materials from plants, where do these *testacea*,—creatures, for the most part, fixed to the bottom,—find their vegetable food? Doubtless, it will in the end be found that, even here, some kind of vegetation still survives; plants floating, perhaps, in shapeless and imperfect forms, that elude our present means of search, and that catch a feeble spark of life, from the last few rays that find their way to these regions of perennial gloom.

Perchance, too, we may discover some clue to the solution of this enigma, in the presence of some of those singular species of infusoria, which, by virtue of their green parts, share with plants in the power of decomposing carbonic acid, and organising dead matter; while, in all their other functions, they resemble other animals,—curious creatures of bi-fold life, which recent researches tend to show, are employed on a vast scale in the living economy of the globe. However this may be, when we reflect that light is the very life of plants, and its agency the first condition of their existence, this discovery of animals in regions, in which we know eternal darkness reigns, must, at least, be regarded as one of

peculiar interest at the present time. The fact naturally leads us to the last subject to be treated of here, namely, the influence of light on vegetation. Among the many important investigations lately made in this matter, those by Dr. Draper have the most interest, in relation to the point under consideration. This distinguished observer justly remarks, that the investigation of the decomposition of carbonic acid by leaves has, by its connection with modern chemistry and physiology, acquired extraordinary interest. For, if we bear in mind, that this decomposition is the starting point of the organisation of dead matter; that, with this agency of leaves, the series of organic atoms begins, which becoming more and more complicated as they pass through the plant, finally end in flesh, blood, and nervous matter; it is evident that correct views on this first conversion must be of no common importance.

Now, Dr. Draper has shown by accurate experiments, and in opposition to what might have been surmised, that the elements of the spectrum concerned in this agency, are not the heating, nor the chemical, or, as he calls them, the "tithonic," rays,—for the help which these give is quite insignificant,—but the rays of highest illuminating power. That it is light, itself, in fact, in the common acceptation of the term, that is the great creative power.

It would lead me too far from the scope and objects of this Report, to pass in review the kindred researches of Herschel, Melloni, Talbot, Hunt, and others. Suffice it to remark, that at this point physiology connects itself with the most beautiful investigations of our time,—investigations from which will date a new era in *physics*, and which, in the discovery, in the sunbeam, of agencies so widely differing in their sensible qualities, and in the nature of their effects, but yet identified by common properties of refraction and polarisation, already seem to point to that great consummation, which the most eminent philosophers of modern times have ever anticipated as the ultimate and highest generalisation of science—namely, the blending all the imponderable agencies of the universe into one essence.

What sublime views here open before us! Already a beautiful truth broke upon our minds, in the discovery, by Dr. Draper, that the same agent which brings in its beams the witness, at once,

of countless worlds at infinite distances, and of the objects immediately around us, in all the rainbow hues with which itself has decked them, should also be God's great dispenser of life, and first cause in the work of organisation. Withdraw its lovely and benignant influence, and the earth would be, again, without form and void, and would float through space a lifeless corpse.

It is only when we strive, as far as our limited imagination will allow us, to contemplate this radiant and ethereal messenger, in all these its wonderful attributes, that we can rise to some vague sense of the vast moment of that mandate, which went forth at the beginning of the world, and which is announced in Holy Writ in these words of sublime simplicity:—

AND GOD SAID, LET THERE BE LIGHT; AND THERE WAS LIGHT.

THE

VARIETIES, CAUSES, PATHOLOGY, AND TREATMENT,

OF THE

INFLAMMATORY AFFECTIONS OF THE RETINA.

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An important variety of amaurosis is dependent on inflammation of the retina, which may occur as an idiopathic, or as a secondary affection. It is, doubtless, rare as an acute, idiopathic, and uncomplicated disease, but comparatively very common as a derangement originating simultaneously with iritis, choroiditis, or general internal inflammation, or primarily set up in the surrounding textures, and involving the retina in its progress; under which circumstance the retinal affection forms a prominent part of the general disease. It is owing to the frequency of this complication that imperfection or loss of vision so generally attends the progress and termination of iritis, choroiditis, &c.

One great drawback to the knowledge we might otherwise attain in the more obscure diseases of the eye, is the rarity with which they afford any opportunity of making a *post-mortem* examination during the progress of the disease, or shortly after the tissues have suffered organic changes, so as to connect the detectible alterations with the symptoms during life. The retina, however, although invisible during health, becomes visible during disease; and the changes which it undergoes from loss of transparency, effusion of lymph, displacement, &c., are visible, and can readily be appreciated by the practised and careful observer. In fact, we possess in the

eye a privilege of following the mode in which nature proceeds—of tracing the commencement, progress, and completion of pathological changes, and the effects of treatment, in a manner which we cannot do in any other organ. No other part of the body affords so favourable an opportunity of studying the process of inflammation in all its forms, stages, and degrees; of tracing the influence of various constitutional conditions on local disease; of observing the modifying effects of tissue; and of actually inspecting and following the immediate and remote consequences of treatment.

Idiopathic retinitis, as I have already remarked, is a comparatively rare disease of the eye. Of its various forms, the chronic are the most numerous, the active acute varieties the most rare, whilst the intermediate or mild cases possess an intermediate frequency. In order, however, to arrive at a correct conclusion concerning the relative frequency of retinitis to the other diseases of the eye, it will be necessary to refer to something more definite than mere general statements. I will, therefore, examine the statistical reports of three great ophthalmic institutions—the Glasgow, Birmingham, and West of England Eye Infirmaries. This last infirmary I had the opportunity of attending for several years. About 1000 new patients, including in-door cases, are admitted in the twelve months, and from 1300 to 1400 cases, including those whose names remain on the books after the statistical list is made up, treated annually:—

WEST OF ENGLAND EYE INFIRMARY.

Years.	Total Number of Cases admitted.	Cases of Retinitis.	Acute.	Chronic.	Proportion.	Ditto of Acute.	Ditto of Chronic.
Oct. 1837 to Oct. 1838 ..	1004	5	...	...	1–200th	. .	...
Oct. 1838 to Oct. 1839 ..	957	4	1	3	1–240th	1–957th	1–320th
Oct. 1839 to Oct. 1840 ..	1059	2	1	1	1–530th	1–1059th	1–1059th
Oct. 1840 to Oct. 1841 ..	906	9	4	5	1–100th	1–226th	1–186th
Four Years...	3926	20	6	9	1–200th	1–500th	1–330th

BIRMINGHAM EYE INFIRMARY.

Years.	Total Number of Cases admitted.	Cases of Retinitis.	Acute.	Chronic.	Proportion.	Ditto of Acute.	Ditto of Chronic.
1836	809	2	...	...	1–404th	...	...
1837	872	8	...	...	1–109th	...	...
1838–9	2229	13	...	...	1–185th	...	...
Four Years...	3910	23	...	...	1–170th	...	...

GLASGOW EYE INFIRMARY.

Years.	Total Number of Cases admitted.	Cases of Retinitis.	Acute.	Chronic.	Proportion.	Ditto of Acute.	Ditto of Chronic.
1839	966	...	...	...	...	...	...
1840	966	1	...	...	1–966th	...	...
1841	844	...	...	...	...	...	...
Three Years ..	2776	1	...	...	1–2776th	. .	...

It is most probable that only the more acute cases were placed under the head of retinitis, at the Glasgow Eye Infirmary; and all the other forms were included under that of amaurosis. This would, in some measure, account for the fact of only one case being recorded in three years.

At the London Ophthalmic Hospital, all forms of retinitis are placed under the division of amaurosis; and, on this account, I am sorry to say that I am unable to compute the relative frequency of this disease in that institution. Mr. Dalrymple, however, informs me that they do not see more than two cases of really acute retinitis during the twelvemonth.

By referring to these sources, it will be found that the opinions I have already expressed are very near the truth. Thus, by the returns of the West of England Eye Infirmary, which I know to be correct, there were twenty cases of retinitis admitted, out of 3,926 patients, during four years, namely, from October, 1837, to

October, 1841, inclusive; being one case of retinitis in every 200 examples of other ophthalmic diseases. Of these, six were acute cases, nine chronic, and five not specified; so that the acute form occurs about once in every 500 cases, and the chronic about once in every 330. A very chronic form of retinitis, which I shall have to describe hereafter, is not classed as retinitis at all, but placed under the head of amaurosis at this institution, which is most probably the case at the other ophthalmic infirmaries; so that the proportion of the chronic varieties is underrated. The relative proportion of retinal inflammation appears to be greater at the Birmingham Eye Infirmary, namely, about one case, not specified whether acute or chronic, in every 170 cases of ophthalmic disease. If, however, we combine with these the results of the Glasgow Eye Infirmary and the London Ophthalmic Hospital, acute retinitis, on the average, would only occur once in every 1755 cases of the other diseases of the eye. My own experience would lead me to say that this last calculation is a fair average of the most acute cases; of the sub-acute, one in 500; and of the chronic, one in every 330.

Idiopathic retinitis presents many varieties in its duration and symptoms. It may be an acute, sub-acute, or chronic affection; or be active, sub-active, or passive, in its symptoms. I restrict the terms acute and chronic to the *duration* of the disease, and mean to denote no character it may possess; for however slow an inflammation may be, it still may be active in its characters; or, however rapid, it still may be a passive affection. On the contrary, it will be obvious that the terms active and passive denote only the *character* of the inflammation, and hence have no reference whatever to its duration. There are, however, here, as in all other examples of inflammation, intermediate grades of all descriptions, from the most acute to the most chronic, and from the most sthenic to the most adynamic. We shall not be surprised, therefore, at meeting with cases in which distinctive features, either active or passive, do not exist; and some where the affection is comparatively mild and slight.

Retinitis rarely attacks both eyes simultaneously; indeed, as an acute affection, I have never seen such an occurrence, but I have several cases on record where both eyes were either simultaneously or consecutively affected in the chronic form of disease. It is rare

for the inflammation to continue confined to the retina during the progress of retinitis; but, sooner or later, the hyaloid membrane, the membrane of the pigment, the choroid coat, iris, or sclerotica, become involved, and, more or less, organically changed. I will now pass on to consider the symptoms, causes, pathology, and treatment, of the various forms of retinitis, under the division of acute and chronic.

ACUTE RETINITIS.

Active Form.—In the most severe cases, acute deep-seated pain of the globe, with an agonising feeling of tension and pressure, which is increased in paroxysms, ushers in the disease. The pain is invariably aggravated towards night, and is accompanied by distressing and almost intolerable pain over the brow, which extends to the head, and frequently shoots or darts through the brain. The eye is exceedingly tender and intolerant of pressure; light is insupportable; and there is profuse lachrymation from any source of disturbance, and on every attempt to examine the organ. When we come to separate the lids in order to examine the eye, we find that the orbicular muscle is spasmodically contracted, as in strumous photophobia, especially if the patient be exposed to the influence of powerful light. Very little redness of the sclerotica attends the commencement of the affection: a slight zonular arrangement of pink-coloured straight vessels, of a very minute and fine size, surround, but do not reach, the cornea; and the conjunctiva exhibits a few dilated trunks of a dark purple or, in some cases, bright scarlet colour, which pass towards the cornea over the globe from the recti muscles. Vision is greatly impaired from the very commencement, and so painful, from the intolerance of light, that the patient is quite incapable of using his eyes: he complains also of the sensation of vivid flashes and scintillations of fire passing before his eyes, sometimes of a red or orange colour, and other morbid appearances of light.

Photophobia alone, without zonular redness and symptomatic fever, cannot be considered as an indication of inflammation of the retina. Mr. Travers* has observed "that the retina is sometimes, though rarely, the seat of inflammation; but it is an error to

* *Synopsis of the Diseases of the Eye;* second edition, p. 137.

suppose that intolerance of light is a sign of this affection, as is clearly proved in strumous ophthalmia, in which, though the intolerance is in excess, the retina is uninjured: and secondly, because the effect of inflammation upon a nerve of sense is to produce palsy, not increased excitability." The fact is, that most probably the intolerance of light depends on the fifth nerve being affected in both instances; either the filaments distributed to the lids, conjunctiva, and iris, or that in connexion with the retina tself. Moreover, Mr. Travers is evidently deceived in his second proposition; for all the best observers of ophthalmic disease concur in stating, that an increased sensibility to the stimulus of light forms a prominent feature in the first stage of retinitis, although the solitary occurrence of photophobia in amaurosis by no means characterises inflammation of the retina.

In the second stage of the affection, the pupil loses its clear black colour, and the aperture of the iris becomes even more contracted than during the commencement of the disease, until, at a later period, sensibility is diminished, when the pupil becomes dilated proportionately. In some cases the iris is discoloured, and manifests a tendency to bulge towards the cornea, while the pupillary margin is retracted. As the disease proceeds, the symptoms increase in violence; the zonular arrangement of the sclerotic vessels becomes more marked, although, whilst the inflammation is confined entirely or mainly to the retina, there is never present any great amount of vascularity affecting either the conjunctiva or sclerotica; and the headache increases considerably;—it is said by some observers to increase to an almost insupportable and maddening extent. Dr. Mackenzie* remarks, that so severe are the sympathetic fever and headache which attend retinitis, that it somtimes passes with medical men, who have not studied the diseases of the eye, for phrenitis or brain fever, the characteristic symptoms of the ophthalmia, from which the affection of all the other parts arises, not being sufficiently prominent to arrest attention.

By and bye, the retina, from having been acutely intolerant of the presence of light, becomes nearly insensible to its stimulus. The pupil dilates widely, and ceases to act from alterations in the quantity of light; organic changes have been induced in the retina,

* *Practical Treatise*, &c.; second edition, p. 545.

which have destroyed its sensibility. Where the other tissues of the internal eyeball have become simultaneously or consecutively diseased, we find, with these changes in the organic constitution of the retina, that the quantity and quality of the pigmentum nigrum is altered, the vitreous humour is in a dissolved state, and the hyaloid membrane more or less destroyed. The pupil loses entirely its normal blackness and brightness, and presents a turbid greenish opacity, which is frequently complicated with cataract, which is itself of a greenish tinge. When their changes are complete, and the green colour evident, they constitute acute glaucoma (γλαυκος green).

Inflammation originating in the retina sometimes spreads to all the other textures of the globe, accompanied by very severe conjunctivitis and chemosis (ophthalmitis). Not unfrequently, under these circumstances, suppuration takes place; the globe is converted into a complete abscess; the cornea or the sclerotica gives way, more commonly the former; the humours escape, in some instances, indeed, the whole contents of the globe are evacuated; the tunics then collapse, become completely atrophied, and are buried deeply in the orbit, being covered in by the lids.

When the inflammatory process is very acute, these changes may be induced in an exceedingly short space of time; even under a more favourable and milder form of the inflammation, the sight is but too frequently and irretrievably lost in the space of a few days, should the affection have been neglected or improperly treated. Mr. Travers states, indeed, that he has seldom seen an example of retinitis which seemed to afford time for the beneficial operation of a remedy.

Retinitis, moreover, may terminate in resolution at various stages of its progress, either spontaneously, or by active and efficient treatment, leaving amaurosis, more or less complete; a dilated and immovable pupil; a discoloured irregular iris; and a condition of the globe, on the one hand, either sunken, flaccid, and flabby; or, on the other, tense, hard, and inelastic,—a state which is generally combined with a glaucomatous condition of the humours and lens.

Acute retinitis, as I have already shown, is a very rare disease; but, as an independent, idiopathic, and uncomplicated affection, where none of the other tissues are in the least degree involved, it is probably still rarer. On the other hand, I think it by no

means unlikely that the retina participates, in a greater or less degree, in almost all the inflammatory affections of the globe; and that Mr. Lawrence's remark,* that "the retina may be not much less liable to the inflammation than the iris, but that we are less acquainted with the phenomena, because the part affected is out of sight, and the disease does not terminate fatally," is true. Mr. Wardrop† also,—who, however, seems to consider an increased sensibility of the eye to the stimulus of light as an indication of retinitis,—observes, that "the retina generally participates in most cases of ophthalmia, in whatever texture of the globe the inflammation commences; the degree," he adds, "being always indicated by the increase of the sensibility of the eye to light."

Constitutional Symptoms.—Symptomatic, inflammatory, or sthenic fever, in various degrees, always accompanies the progress of acute active retinitis. The patient feels languid, restless, and incapable of any exertion; the skin is hot and dry, especially that of the extremities; the pulse is hard, sharp, and rapid, or full and bounding; the tongue is loaded with a thick white, or yellowish crust; the stomach is foul, nausea and vomiting being sometimes present; the bowels are confined; the mouth is dry and clammy, and the patient often suffers from thirst. All these symptoms are aggravated by a low position of the head, heat, and improper diet. It is stated, that in some cases so great is the constitutional disturbance, so agonizing and unbearable the headache, the intolerance of light so excessive, so anxious the expression of the countenance, and so great the nervous excitement of the system, whilst the local symptoms are comparatively undeveloped, as closely to simulate the severer and more dangerous disease of meningitis. It would seem probable, that retinitis is in reality sometimes combined with meningitis, as a complication.

The burst of retinal symptoms occurs suddenly, but is almost always preceded by constitutional disturbance. For some time previously, the patient has been languid and listless; complains of lowness of spirits and despondency; has restless nights, anorexia, headache, nausea, vomiting, or disordered bowels; the tongue is coated, the general surface cold, and the patient is chilly. Mr.

* *Treatise*, p. 323.

† *Morbid Anatomy of the Eye*, vol. ii., p. 157.

Tyrrell* believes retinitis to be always a consequence of some material derangement of the general or constitutional power, under which the balance of the circulation is easily disturbed: thus, in the history of cases of this inflammation, we find usually that the patient has been observed, by those immediately about him, to have been unwell for some time previous to the commencement of the retinitis. The general symptoms become more manifest when the local disease is developed, and new symptoms present themselves as consequences of the retinitis.

Dr. Jacob,† however, has most judiciously remarked, that it would be dangerous in practice to suppose that inflammation of the retina is always accompanied by these violent symptoms. He says, that the disease is often mild and insidious in its approaches, and marked more by defective vision than by symptoms of inflammation. In this statement I fully coincide.

Very acute, active retinitis I believe to be a very rare disease indeed. Most inflammatory affections are described as if they presented the most intense symptoms, and required the most unsparing and active use of the lancet, backed by the whole catalogue of anti-phlogistic remedies. Nature, however, by no means furnishes us with such cases as her invariable rule; nay, such cases are the exceptions, and not the type of any one inflammation. Most inflammations occur in individuals whose constitutional powers have been deranged previously; and in proportion to the kind and degree of this general indisposition, so is the process sub-active, mild, or marked by failing powers, constituting the atonic, where the fever present is not "inflammatory," but of a lower grade, with every intermediate link to complete prostration and typhoid fever.

I know nothing practically of that most acute form of retinitis in which the symptoms can scarcely be diagnosed from inflammation of the meninges of the brain, and where there is no time for the beneficial operation of a remedy. I should imagine that these descriptions were rather heightened, as such severe head symptoms, and such extreme rapidity, were not marked in any one case which I have seen. As I have preserved the notes of several cases which have come under my own observation, I will select a few, to give

* *Cyclopædia of Practical Surgery*, Part I., p. 81; Article, "Amaurosis."

† *Cyclopædia of Practical Medicine*, vol. i., p. 60; Article, "Amaurosis."

what I believe to be the actual facts of the case. In some of the cases I have witnessed, active fever preceded all local symptoms; but this was by no means invariable.

Case 1.—I saw the patient a few hours after the commencement of the attack. She went to bed in good health, according to her own account, and awoke early in the morning with deep-seated pain in the eyeball, severe distress over the brow, general indisposition, and almost complete loss of vision. About noon the same day I saw her, and she then complained of the same symptoms, with great intolerance of light, sensations of flashes of fire before the affected (right) eye, and tenderness of the eyeball. On examination, there was little to attract attention in the various textures of this eye, except a slight halo of very fine straight vessels around the cornea. The constitutional symptoms, on the contrary, were well marked: there was considerable fever of the inflammatory or sthenic type present; a hard, full pulse; hot, dry skin; a coated tongue; a clammy state of the mouth, and thirst. The patient was a full, plethoric woman, and held the situation of cook in a gentleman's family. This case, with another example I will briefly sketch, leads me to believe that great impairment of vision is one of the first local symptoms experienced.

Case 2.—A fair, florid young woman, named Eliza Harwood, aged 21, of sanguine temperament, and possessing many of the appearances characterising the strumous constitution, applied at the West of England Eye Infirmary, in the spring of 1838, suffering from symptoms which she stated had been brought on a few days previously by a flash of lightning, whilst employed at needle-work near an open window. On examining the eye, we found scarlet zonular vascularity—not reaching the corneal edge, but leaving a white circular line intervening; the vessels very small and straight; the zone well formed, but not intense. She complained chiefly of almost complete loss of vision, and of false appearances of light before the eye. There was great pain also in the head and eye, inflammatory fever, intolerance of light and lachrymation to an excessive degree, slight inflammation of the conjunctiva, and complete freedom from any inflammatory appearances of the cornea or iris. The attack was subdued by blood-

letting, purging, and mercury, followed by counter-irritation, and she perfectly recovered her vision in this eye for the time. She, however, presented herself again, several months afterwards, with a functional form of amaurosis in the same eye, evidently dependent on a disturbed condition of the uterus, which proved most obstinate.

This patient stated that she had always been of healthy and active habits, until a short time before the occurrence of the retinitis, when she had been engaged in the employment of a large factory. Her irides were light coloured.

I have introduced these portions of cases especially to show the incipient and early symptoms, and will now copy from my note book a very interesting example of the same disease at a later period of its existence, which proves that judicious measures are capable not only of putting a stop to the symptoms, by curing their pathological origin, at an early period of their existence, but even of restoring vision after its complete loss, provided that treatment be commenced before the functional derangement has become permanent, or the effused lymph unsusceptible of the influence of mercury, by its becoming organised and undergoing analogous changes.

Case 3.—*Retinitis of the Left Eye, of the Sub-acute Active Type; Buff Discolouration of the Pupil, from Effusion of Lymph into the Texture of the Retina; with complete Loss of Vision, and a Soft Flaccid State of the Globe; complete recovery, from the use of Mercury.*—A fine healthy girl, called Mary Ann Saunders, aged 19, presented herself as a patient in May, 1840. She stated, that previous to the present attack she had had an inflammation of the same eye, which was nearly, but not perfectly cured before the present symptoms commenced. Some uneasiness had existed for three months. Her usual health was very good, but she was nervous, and had previously suffered from hysteria; had occasional pains over the brows, and uneasiness in the left side.

The previous symptoms commenced with pain and "sticking" in the left eye—a feeling resembling sand between the surfaces of the conjunctiva—intolerance of light, lachrymation, and headache. The eye, according to her own account, was of a bright scarlet colour. This attack was treated simply by quiet, abstinence, and warm bathing by poppy fomentation.

At the time of her application the following symptoms were perceptible:—Well-defined zonular redness, very narrow, intense towards the cornea, and imperceptibly shaded off externally; the vascularity forming a complete circle, consisting of the finest ramifications, which terminated by a well-defined line towards the cornea, at a short distance from which they perforated the sclerotic tunic, and thus left a white ring, which intervened between the vascularity and the cornea. The iris of the left (the affected) eye was widely dilated and motionless, but was otherwise healthy in appearance, and the fundus of this globe presented a dull buff-coloured opacity, whilst the schlerotica and cornea were free from discolouration, and appeared quite normal in function and structure. The conjunctiva presented a few dilated scarlet vessels, and the lids were red, painful, and tumid. The globe of this left eye, to which the disease was confined, appeared preternaturally soft and sunken.

From the patient we learnt that this attack commenced about a fortnight before, with pain in the eye and head, dimness of vision, and great general feeling of illness. She stated that her sight, at the time of taking these notes, was completely lost; that she was able only to distinguish "night from day" with the left eye; that she had severe pain in the head, chiefly in the frontal region; great sense of weight and aching over the brow, much increased towards night, and shooting through the brain to the occiput; and that during the day the pain over the brow was aggravated in irregular paroxysms. The pain in the eye-ball itself was less severe than at the commencement of the present affection; it was deep-seated, but not very much. She complained also of the sensation of muscæ, sparks, and the appearance of flashes of light passing before the eye. As regards the constitutional symptoms, her general powers were depressed; the tongue coated and red at the tip and edges; skin dry and hot; there were thirst and nausea, with general feelings of indisposition, depression, and lassitude; the pulse frequent and soft; bowels confined. The diagnosis from these symptoms was acute retinitis, of the sub-active or mild form, the blindness depending on inflammation and change of structure in the retina.

The inflammation was here evidently on the decline; the pain and other symptoms were less violent than at a previous period;

but changes had already occurred, although very recently, by which the retinal function was almost completely abolished; it thus afforded a favourable opportunity for a trial of the effects of mercury.

Accordingly eight leeches were applied to the temple, the alimentary canal was freely cleared out by an active purgative, and then two grains of calomel and one-fourth of a grain of opium were administered at short intervals, so as to bring the system under its influence. A weak, warm, solution of the acetate of lead was directed as a local application to the conjunctiva. (May 5.)

Four days subsequent to the commencement of the mercury the mouth became affected, and she then stated that her sight was much improved, and that the sensations of muscæ, light, &c., were diminished. The pupil was still dilated, but possessed independent motions on the alternation of light and shade; the fundus opaque, tongue coated, lips dry; little or no fever, except at night; local pains gone; redness and swelling of the gums, mercurial fœtor and coppery taste in the mouth; zonular redness much diminished. To continue the mercury in smaller doses; a blister to be applied behind the left ear.

On her next application the mouth was still sore; her sight had improved considerably, the pupil had regained almost its normal mobility, the fundus had lost the buff appearance, and was now scarcely opaque. The blister was discharging, and it was directed to be kept open, and the previous remedies continued.

A week subsequent to this period the patient had lost every diseased appearance and sensation, and declared her sight to be as good in the left eye as in the right, but "weaker,"—namely, more impatient of exercise. As the lids were still somewhat red, an astringent collyrium was now employed. The patient ceased attending.

Now, I would inquire, are the incipient and confirmed symptoms, which occurred in the cases I have narrated, the ordinary indications of acute retinitis?—or do the constitutional symptoms, &c., so nearly simulate phrenitis, and are the local symptoms so equivocal as to render the diagnosis difficult? Considering the rarity of this disease I have seen a large number of cases, (as many as eight of

the acute forms,) and but two where the progress was more rapid and active than in the case of Saunders; but in all, the symptoms exhibited a great similarity: the vascularity was of the same type, the primary contraction and dilatation of the pupil, attended with opacity, or a buff-coloured change at the fundus of the globe. The fever and constitutional symptoms were also similar, but with such trifling derangement of the cerebral functions, that no difficulty could, at all events, have been experienced in any of these. I find, also, that the most trustworthy cases of acute retinitis on record correspond with my own experience, both as to head symptoms and rapidity of progress.

Dr. O'Beirne, of Dublin, has published a very interesting example of retinitis, affecting both eyes, in the seventeenth volume of the *Dublin Medical Journal*, p. 230. The patient's name was Bridget Johnstone, aged 28, a servant. In this case there were violent darting circumorbital pains, and pain in the balls of the eyes, which were always worse at night. Accompanying these symptoms were frequent flashes of light and sparks of fire crossing, and motes continually flying before her eyes; occasionally two red globes and small balls of fire would appear at a very short distance from her bed or wherever she was seated. At night the light of a candle would assume a variety of colours—green, blue, redder than natural, approaching to a deep scarlet, or crimson hue, and at times an intermixture of the first three. At the time of her admission both pupils were dilated as much as could be imagined, and were quite insensible to light. The lens and humours were perfectly transparent, but the bottom of the eye presented a glaucomatous appearance. She was admitted on the 24th of December, 1839, and her vision was completely restored, by a full constitutional action of mercury, by the 4th of January following.

An interesting case of acute retinitis, with temporary, but complete loss of sight, caused by lightning, is recorded in the second volume of the *London Medical Gazette*, pp. 58–9. This case displays the power of mercury when actively employed, and the absence of any severe head symptoms, which could by any possibility mislead the practitioner.

Mr. Tyrrell has also published several cases; but none of these presented any phenomena at all approaching to the description of Mackenzie, &c.

Acute retinitis is not always confined to one eye, as Dr. O'Beirne's case, and the experience of Mr. Tyrrell fully prove, although, as I have previously stated, I have never seen the acute form of the disease myself affecting both organs.

Passive Forms of Acute Retinitis.—When retinitis occurs in elderly persons, or where the nervous system has been much depressed by mental anxiety and protracted distress; also where the whole constitution has been much deranged by previous disease; and, lastly, where the original make of the body is feeble;—the local phenomena are apt to display much and disorganising action, without power, and other constitutional symptoms are of a low type. These are very unfavourable cases, having in themselves a greater tendency to an unfavourable termination, and being less under the influence of medicine.

In these cases we meet usually with much anxiety and irritability of mind, much languor and depression; the pulse is soft and feeble, sometimes jerking, but readily compressible; the skin dry, but without much heat; and the countenance indicative of depression and anxiety rather than of excitement and fever. The difference between Mr. Tyrrell's experience and my own is, that whilst I should say, from the cases I have seen, that the passive forms of retinitis were the exceptions only, Mr. Tyrrell considers them the ordinary, and more active types the exceptions to a general rule. The most rapidly destructive and distressing cases that Mr. Tyrrell has witnessed* have occurred during great mental anxiety and distress; as in the elderly female, from the loss of husband or child, or of some near and dear friend; in the male, from sudden loss of property or comfort, in consequence of imprudent speculation, or from the deceit of one previously esteemed and valued.

Causes.—The class of patients who are most frequently affected with retinitis, are those who from their employments and mode of life are predisposed to inflammatory attacks, either active or passive. The previous occurrence of the disease powerfully predisposes to a recurrence on slight occasions, and the presence of chronic disease is sometimes lighted up into acute inflammation. Females appear to be much more liable to the affection than males, and, as far as I

* *Practical Work on the Eye*, vol. ii., p. 150.

have seen, those under middle age. Mr. Tyrrell, however, has seen the disease most frequently between the ages of 45 and 60, and occasionally in persons of more advanced life. He says, also, that it is almost confined to persons having dark irides. My experience is to the contrary. Females residing as cooks in families, who live freely, drink proportionately, and are exposed to much heat and the light of large fires, frequently suffer. I have already mentioned a case of this kind; and Mr. Lawrence narrates several in his *Treatise on the Eye*. Of the seven cases he gives, in the section on retinitis, five occurred in young women, some of whom he particularly mentions as having light hair and eyes; the other two in young men, in one of whom it was induced by lightning, and the other was a carman, accustomed to drink freely. One of Mr. Lawrence's cases* is as follows:—

Case 5.—*Retinitis in a Cook, from exposure to Heat and Light.*—A young woman, of florid complexion and full habit, came to the London Ophthalmic Infirmary, complaining that she had lost the sight of one eye. She was a cook in a family, and occupied for several hours daily before large fires, supporting her strength by free living. The pupil was slightly dilated, the iris motionless; a faint and scarcely perceptible pink tint was observed in the sclerotica near the cornea; vision was dim, and had been so for three days. There was headache, flushed countenance, heat of skin, whitish tongue and thirst. Mr. Lawrence considered the case to be pure retinitis, and to afford a favourable opportunity for showing whether the affection could be arrested by anti-phlogistic treatment. At that time (now many years ago) he did not possess the knowledge of the powers of mercury in inflammation of the retina, which subsequent experience has given him. He directed a full bleeding from the arm, free purging, low diet, repose of the organ, and general rest. At the end of two days the sight was worse; cupping and a blister were now ordered, but there was no improvement at the end of two days more. He now determined on trying mercury, and ordered two grains of calomel every four hours. Before the remedy had affected the system vision was quite lost, or at least reduced to the mere power of distinguishing light from darkness. Full salivation, which took place in about a week from the first

* Second edition, p. 480.

application of this patient at the Infirmary, suspended all the symptoms; sight immediately improved, and was soon completely restored.

Idiopathic retinitis, then, usually comes on in predisposed individuals, whose health has been deranged for some time antecedently, from causes which produce excitement in the retinal circulation. An occasional exciting cause, as in other inflammations, is exposure to damp and cold. In Dr. O'Beirne's case, the patient took "cold" by having her clothes greatly reduced in quantity, and her shoes and stockings taken away, and was almost immediately seized with the incipient symptoms. Again, any sudden and very powerful stimulus, as light, may induce the disease. When the retina is powerfully stimulated, either by excess of vivid light, or by being strained by the careful inspection of very minute and brilliantly illuminated objects for any length of time, one of two very opposite pathological conditions may be induced—retinitis,* or atonic paralysis. Thus the sudden impression of a flash of lightning may induce an immediate and perfect abolition of the retinal function, or the most active and violent retinitis. The artist who was engaged in taking the panoramic view of London, from St. Paul's, for the Colosseum, was intently inspecting some object by the aid of a camera lucida, when the transit of a dark cloud allowed the brilliant light of the sun to fall unexpectedly upon the retina in the axis of vision, concentrated by the instrument. The effect was instantaneous palsy of that part of the retina, which did not recover its function, whilst that of the surrounding parts remained perfect. I have already given a case of retinitis produced by lightning, and will now narrate the case published by Mr. Mayo, in the *Medical Gazette*, vol. ii., pp. 58–9.

Case 6.—Jane Humphreys, eleven years old, was standing in a

* *Vide a Case of Acute Retinitis, caused by the use of the Microscope.* By W. W. Cooper, Esq., Surgeon to the North London Ophthalmic Institution.

The patient, Mr. G——, was dissecting the nerves of the tongue, under a powerful microscope. "The nerves having been clearly dissected, were of a dazzling white; and whilst he was intently regarding them through the microscope, the sun—which had previously been obscured—suddenly shone forth in all its brilliancy upon them. Acute pain was instantly felt in the eye." Having incautiously used the organ on the following day, retinitis succeeded. Judicious treatment having been employed, "the eye gradually and steadily recovered, and is now perfectly well."—*Lancet*, July 6, 1844.

school-room, with her left side towards the window, on the 6th of May, 1828, when a storm came on, and the room was strongly lighted up by a flash of lightning, which produced instantaneous loss of sight in the left eye, with tingling pain in the globe. As the pain increased during the following days, Mr. Mayo was sent for. Repeated leechings on the temples, blisters behind the ear and on the back of the neck, with mercury, so as to affect the mouth, were employed by his directions: under this treatment there was visible daily progress towards recovery.

The symptoms, on the 11th of May, were, sense of heat in the eyeball, and tenderness on pressure; inability to raise the upper lid; extreme sensibility to light, when the eyelids were held open; vision almost extinct. There was no increased redness, nor any change in the transparent media. Pain and tightness across the forehead; throbbing in the head; white tongue; frequent pulse.

May 20.—Objects could be distinguished; total want of power to raise the upper lid; she could bear the light when the lids were opened.

May 24.—The power of the levator palpebræ superioris was restored.*

May 27.—Although the left eye appeared sound, and was used habitually with the other, it was weaker than before, and somewhat painful when exercised. She could read ordinary print with it when held near the eye, or distinguish a pin and pick it up from the ground; but the faculty of distinguishing colours was lost. In a yellow silk handkerchief, spotted with scarlet, she could point out the spots, but they appeared to her black, and the ground less black. She described white paper as a shade of black, and the leaves and petals of a rose as a deeper shade.

The powerful reflection of light from a tract of country covered with snow, has been known to induce retinitis in travellers. The Esquimaux, who inhabit Hudson's Bay, are so well aware of the loss of vision which arises from the exposure of the eyes to the

* I shall presently give the particulars of a case of retinitis, which came on apparently in connection with a severe blow on the side and arm, but without the slightest injury of the head, in which partial ptosis was a prominent feature. In the present case of Mr. Mayo's, it is quite obvious, I think, that the ptosis was also quite independent of any disease or derangement of the brain. It commenced and disappeared with the retinitis, and was unconnected with any cerebral symptoms beyond the ordinary sympathetic phenomena of retinitis in its acute form.

dazzling brilliancy of a country covered with snow, (snow-blindness,) as to use contrivances that act as preservers, which they call snow eyes. The snow eyes are made of wood or ivory, which are so formed as to fit and completely cover the eyes, and are fastened behind the head. In the centre of each piece there is a narrow slit or aperture, which is usually horizontal, through which they see distinctly.

Trnka,* Monro, Petit, and Winslow, assert that a strong sympathy, (it is, in fact, an actual connection,) exists between the great intercostal or sympathetic nerves and the eye, especially evident with the iris and pupil. Anatomy reveals an actual connection, (for nerves do not anastomose, or their fibrils join the actual structure of one another,) between the carotid plexus, through the medium of the cavernous plexus, and the third, fifth, and sixth nerves: and to the retina by a filament which accompanies the central artery. The following case came on, apparently in connexion with a severe injury of the side and arm:—

Case 7.—*Sub-acute Active Retinitis of the Right Eye, coming on in apparent connexion with an Injury of the Side.*—John Peak, aged 27, was admitted in September, 1841. He is a powerful, active, healthy-looking man, who, according to his own account, had always enjoyed good health, except from the effects of accidents. He is a farmer's labourer, and his employments have been chiefly in the open air. About ten weeks since he met with a severe accident. It happened, he says, in the following manner:—that he

* Nervorum remotiorum cum oculo consentientium læsiones vel solæ irritationes præprimis vero nervorum intercostalium magnorum. Vero eruditi, inquit A. Monro, inter se non conveniunt, qualem nervus intercostalis habet originem, dum alii ejus communicationem cum ophthalmico negant, alii vero contrarie sentiunt, ejusque connectionem cum hocce adfermant: sic quoque Winslowus aperte dicit ophthalmicum per unam alteramve fibrillam cum sexti paris nervo et cum nervo intercostali communicare. De ejus tamen origine disputare animus hic non est, neque plura quæ a quibusdam pro incerto habentur proferre, quatenus ad observationem nostram non multum faciunt, et nobis, uti ex Petiti observationibus constat, sufficit, maximum inter intercostalem et illos nervos dari consensum, qui oculo, uveæ, atque pupillæ inserviunt, sed tantum ea, quæ majoris utilitatis sunt adnotabo, ex experimentis enim Petiti memoratis supra adnotam est, post nervi intercostalis abscissionem in animalibus vivis factam oculos obscurari, splendorem suum amittere, lachrymare, cavosfieri, globumque oculi diminui, pupillam angustari, coarctari, &c. Denique intercostalem ramos conjunctivæ, glandulis oculorum, et fibris uveæ, quibus pupilla dilatatur suppeditare.—Trnka: *Historia Amauroseos*, vol. i., pp. 297–8–9; 2 vols., 8vo., 1781.

was working with a pickaxe at a high bank, when it suddenly fell in upon him, and severely bruised his right side and arm. About two months since, (a fortnight after the accident,) he found the sight begin to fail in the right eye, without any obvious cause. His vision failed gradually; he became impatient of the influence of light, and frequently saw sparks and flashes of light before the eye, with severe pain and tenderness in the globe itself. He had frequent headache and pain over the brow—the latter symptom invariably increasing towards night. The pain and dimness of vision principally attracted his attention. He attributed the ophthalmic affection to the previous injury of his side: at all events, he knew no other cause which could possibly have occasioned it.

There were still marks of injury in the situation of the parts struck by the fall of the earth. He had received a severe blow over the whole posterior part of the right side of the thorax, nearly as far back as the spine, and on the right lumbar region. Marks of injury existed immediately in front of the prominence of the internal condyle of the humerus; but he positively denied having received any injury on the head, or about the face; and no marks of such were anywhere visible. The effects of the blow on the side had nearly passed off, but not so that of the arm. He complained of shooting pain immediately external to the internal condyle, and in the course of the ulnar nerve, affecting the little and half the ring fingers; of muscular weakness of the fore-arm, and almost complete loss of sensation of the inner and front part of its integuments.

The eye appears somewhat sunken, and feels flaccid to the touch, whilst the fundus is rather more opaque than natural, from a deep-seated buffy discolouration; the pupil is considerably more contracted than its fellow, and possesses scarcely any motion on the alternation of light and shade; the iris is of a light blue tint, and possesses the same shade exactly as the opposite eye, and its pupillary edge is quite healthy. A slight zone of very fine, straight, claret-coloured vessels surrounded the cornea, especially marked at the upper and lower parts of the inner canthus, and several enlarged scarlet vessels ramified in the conjunctiva oculi. The upper eyelid of this eye hung rather lower than its fellow, and the patient was unable to elevate it,—an affection which came

on with the disease, and had not previously existed; it was but slight, covering only about the upper half of the pupil.

The patient complained of considerable imperfection and dimness of vision, which, he stated, was always worse in the morning, when the dropping of the lid was also more considerable. When he closed the left eye, objects appeared misty, distorted, and confused, with the right. There was pain in the globe, which often assumed a dull aching character; and he complained of the appearance of flashes of light and coloured sparks passing before the eye; also of occasional but severe pain in the forehead, shooting towards the back of the head; and, lastly, of dull aching pain over the brow, which was always increased towards night. The pain both in the eye and head has decreased lately, but the dimness of sight rather increases.

He has been an in-patient about six weeks, during which time his chest and arm have become much better, but his general health has deteriorated. His tongue is flabby and coated; his appetite poor, and his digestion palled, whilst his bowels frequently require the assistance of aperients; the skin is cool, but dry and harsh, and his feet and hands become hot and burning to his sensations, in the evening.

These evidently were the symptoms of sub-acute active retinitis in its mildest and most chronic form, having proceeded to some slight effusion of lymph into the texture of the retina: the dulness of vision, the opacity of the fundus, and the zonular vascularity, abundantly testified to this, and clearly indicated the employment of mercury. During his stay in the hospital, the acute symptoms of the affection, although never severe, were much relieved, but his vision became more misty and distorted, although the degree of imperfection was not increased, and the intolerance of light nearly disappeared.

On the 20th of September, the symptoms continued unchanged; vision as obscure as before; less appearance in the signs of any advance in disease. To continue the remedies.

October 14.—Better in every respect; fundus of the globe less misty; vision clearer. The patient continued to improve.

Remarks.—In the foregoing case we find the symptoms commencing about a fortnight after the receipt of an injury to the right side and arm, on the same side as the retinitis, to which the

patient confidently attributed the affection, but which injury never had occasioned any disturbance of the functions of the brain, spinal marrow, or, appreciably, of the sympathetic, although the injury was inflicted externally for a considerable distance over the course of the right chain of ganglia. We may propose the questions—1st, Was there any connection between the ophthalmic affection and the receipt of the injury? 2nd, What was the nature of that connection, supposing one to exist? In the arm, the ulnar, internal cutaneous, and perhaps some other nerves, were deranged by the blow; but, as far as direct symptoms went, with the exception of the retinitis and ptosis, there were no other indications of injury to the nervous system. Although, therefore, the retinitis occurred soon after the injury, it would be difficult to assert positively that the sympathetic chain of the right side was affected by the blow, and that the ophthalmic affections depended on this derangement. If, however, the accident was the true exciting cause, (and there was no other apparent,) I can suggest no other explanation of its modus operandi, than its primary influence on the ganglionic chain, and the ultimate reference of this to its extremity in the retina, iris, and third nerve.

As regards the partial dropping of the lid, and the paralysis of the levator palpebræ, the details of the case give but little clue to its explanation, other than that I have suggested above. It may complicate retinitis having a direct local cause, as in Mr. Mayo's case, where there is no evidence that other parts than the retina suffered. Where there is no cerebral disease, it seems to depend on some affection of that branch of the third nerve which supplies the levator palpebræ. In some instances of partial ptosis that I have seen, evident symptoms of derangement or disease have existed in some other parts of the orbital nerves.

The local and general symptoms were such as I have usually observed in this form of inflammation of the retina. In this respect, the case of John Peak coincides with some others which I have already mentioned.

In proceeding with the other exciting causes of acute retinitis, I may mention that violent blows, contusions, and penetrating wounds of the eyeball, by which the retinal structure is injured, frequently produce a more or less complicated form of the affection.

The symptoms which may result from injuries depend, however, much on the condition of the eye at the time of the receipt of the injury, the degree of violence of the impinging body, its peculiar form, whether obtuse or angular, and whether the eye be or be not penetrated, and foreign matters left in the globe. If the body be angular, and does not penetrate, it expends its impetus on the superficial parts of the eye, and gives rise to peculiar morbid actions in them; but if the eye be unprepared for violence, or if the violence be sufficiently great to overcome the natural means of protection, and the inflicting body be obtuse, the injury is sustained by the deeper-seated textures, paralysing their functions, rupturing their blood-vessels, or disorganizing their structures, frequently being followed by active and intractable inflammation, should appropriate prophylactic measures be neglected, or injurious treatment be adopted. When inflammation is induced, under these circumstances, it shows itself in the form of more or less complicated retinitis, which may terminate still deserving such an appellation, or may spread, involving in its progress the various textures composing the globe, or even the appendages, and thus terminate in confirmed ophthalmitis.*

* It would appear that retinitis may follow the operation of couching, as in the following case from Mr. Hey's Surgery:—

May 28, 1772.—Mr. Hey couched both the eyes of Sarah Newscombe. The subsequent inflammation was trifling, and disappeared the third day. *June* 12th, he repeated the operation on the left eye, and performed a third operation the 25th of the same month. The two latter operations were followed by no greater inflammation than the first. The cataract in the right eye, which had been broken at the first operation, disappeared so fast that no repetition was required. When she could distinguish objects in the fields before the Infirmary with the right eye, she was dismissed, with directions to return in about a month, that her eyes might be examined. Upon her return, he was surprised to find that she had lost that degree of sight in the right eye which she enjoyed when she left the Infirmary: yet the cataract had not appeared again, nor was there any opacity to be perceived in the cornea or capsule of the crystalline. The pupil was too much dilated, and the iris did not contract upon exposing the eye to a pretty strong light. In short, the eye appeared to be affected with amaurosis.

Upon inquiry into the cause and progress of this unexpected complaint, the patient informed him that in returning home, when dismissed from the Infirmary, she had caught cold, which had brought on inflammation in the right eye, and a gradual loss of sight. The redness of the conjunctiva had nearly disappeared, but she still felt a tenderness of the eye. From a consideration of these circumstances, he was led to suspect that the complaint was of an inflammatory nature; and, accordingly, Mr. Hey ordered her to be bled immediately, and directed a purgative to be taken

Sometimes we can detect no exciting cause beyond great predisposition, and even this is apparently absent in others. Dr. Jacob,* indeed, supposes that the causes of inflammation of the retina are as various as those of general inflammation of the eyeball; and if we allow that retinitis complicates most of the inflammatory affections of the globe, this is, doubtless, true. He says, that the disease may arise from gout, rheumatism, syphilis, mercury, or typhus fever; or it may be idiopathic, without assignable cause. I shall have to narrate a case of chronic retinitis, which was evidently rheumatic, when treating of the chronic form of the disease. Mr. Middlemore† has seen the disease many times in attorney's clerks, and in persons employed as artists in the offices of architects; and certainly when it was the custom to employ the eyes by the aid of gas light, in the injudicious manner which was too often adopted on the comparatively general introduction of that mode of lighting apartments, the disease was particularly prevalent, though more generally in a chronic form.

Diagnosis.—I have already stated that, as far as I have seen, there is no danger of confounding acute retinitis with inflammation of any portion of the cranial contents; but there is much more probability of the practitioner's mistaking the sudden commencement of this affection for the symptoms of cerebral congestion.

Mr. Lawrence‡ mentions a case of this latter form of amaurosis in a patient who went to bed without complaint, and awoke in the middle of the night quite blind, with great pain in the head, and feeling very ill. In the first case of acute retinitis mentioned in this paper, the patient went to bed in good health, according to her own account, and awoke early in the morning with almost complete loss of vision, feeling of general illness, and severe distress over the brow. The main points of difference, however, between these cases is, that in the hyperæmial amaurosis both eyes were

the following morning. These means afforded the wished-for relief, and the eye was restored to its former state. He saw this patient *February* 27, 1799, (twenty-seven years after the operation,) and she then enjoyed her sight as completely as the loss of the crystalline humour will admit.—*Hey's Practical Observations in Surgery*, pp. 93–94.

* *Loc. cit.*, p. 60.

† *Treatise*, vol. i., pp. 794–5.

‡ *Treatise;* second edition, p. 512.

affected, the globes themselves were free from pain, and there was no intolerance of light; but in the case of retinitis, only one eye was attacked; there was deep-seated pain in the eyeball, intolerance of light, sensations of flashes of fire passing before the eye, and tenderness of the globe. The eyes might be preternaturally vascular in hyperæmial amaurosis, and the patient complain of sensations of light before them, but in retinitis the vascularity forms a delicate halo round the cornea, and the sensation of flashes of light is much more severe than ever occurs from simple fulness of vessels. In complicated cases, where other parts of the eye are inflamed, we judge how far the retina is implicated by the imperfection of vision, not depending on mechanical obstruction to the rays of light; the sensations of muscæ, sparks, flashes of fire, or coloured luminous appearances; intolerance of light, and whether the retina be or be not visibly discoloured, rendered opaque, buff-coloured, or displaced.

Prognosis.—Some permanent lessions of organisation, and consequent imperfections of vision, so frequently result from acute retinitis, even under the most active and judicious treatment, (especially, however, in passive forms, in elderly people, and from severe injuries,) that it behoves us always to use great caution in giving a prognosis. Far better is it, when disease is really dangerous, for a practitioner to state his fears, but still hope for a favourable termination, even when he fully anticipates no final evil, than confidently assure the friends that there is no danger—deceiving himself, and losing credit, should his promises not be fulfilled. The chances, indeed, of a favourable termination of the present attack are very great, provided the patient be seen early, the disease be met by prompt and judicious treatment, and the inflammation be not of the unfavourable types mentioned above; and I have already proved that recent effusions of lymph into the texture of the retina is not necessarily fatal to vision, provided that such effusion does not occur in unfavourable cases, and the lymph has not yet undergone analogous changes. There is, however, great danger from the occurrence of relapses, and the disease ultimately settling down into a chronic form; for, under these circumstances, a renewal of the acute symptoms is readily produced by causes which would have had no disadvantageous effects in a

healthy state of the retina. If we may believe the testimony of some observers, the progress of the inflammation in some of the most acute cases of retinitis is so rapid, that disorganisation has been produced before the remedies have had time to influence the dilated capillaries; indeed, that such cases are not seen before irremediable damage has been effected; and, of course, that the prognosis is most unfavourable; that vision is permanently injured; and that there is no small danger of the induction of general inflammation, suppuration, and subsequent collapse of the globe. When only one eye is affected, the other is in great danger of sympathetic disease, unless the most active preventive measures be employed. For my own part, I have not met with such cases, but still believe that they are of occasional occurrence.

Dr. Mackenzie states* that, as far as he has observed, the prognosis is not unfavourable, if a proper method of treatment be commenced before the pupil is much contracted, or the power of vision greatly lessened. If, he says, vision seem already extinguished, the prognosis is extremely unfavourable; and if the pupil be once closed, even before the retina appears to have become insensible, there is no longer any hope of preserving sight; for even should the pupil re-open in some degree, as it occasionally does on the inflammatory symptoms being arrested, yet it remains small and motionless, and the eye is still blind. Beer saw two cases where vision returned after the inflammation was subdued, but in both a very considerable weakness remained during life, and the patients could read large print only, with much difficulty, and small print not at all.

Pathological Anatomy.—A great many diseased appearances have been detected in the retina, but few in actual connection with the symptoms of acute retinitis. Patients so rarely die at the time, or shortly after, organic changes have been effected, from this disease, that it is impossible to meet with an opportunity of examining the seats of morbid action, in connection with the symptoms, and ascertaining the changes which have been occasioned by the disease itself, uncombined with subsequent alterations. For it is evident, that if the use of the eye be lost, and the tissues be not examined for many years afterwards, its condition at that

* *Practical Treatise;* second edition, p. 547.

time will throw no light on the disease which primarily induced loss of vision. Notwithstanding these disadvantages, however, we can often inspect the changes which are going on during life, almost as well as if exposed by the scalpel.

Mr. Walker* has a preparation of the eye of an ox, in which the retina is seen to be much thickened, and covered over with a dense layer of coagulable lymph, which morbid condition has also extended, in a less degree, to the choroid, ciliary processes, and posterior capsule of the lens; the vitreous humour also was of a decidedly straw colour, and the septa forming its capsule had entirely disappeared.

In examining an eye myself, which had been lost many years, I found no trace of retina, but its place supplied by a cup of an elastic substance, with all the appearances and properties of cartilage; the optic nerve without a particle of nervous matter, consisting of mere neurilema; the vitreous humour deprived of capsule, of a dark brown colour and fluid consistence, and abounding in shining particles of cholesterine; the lens atrophied, of a brown colour, and fatty appearance; anterior chamber nearly obliterated, and the cornea very atrophic. Mr. Langstaff's museum contains, also, some specimens of thickened and puckered retinæ.

Treatment.—Before I proceed to consider individual remedies separately, I would inquire whether we can lay down any one rule, or set of rules, to guide us in the administration of remedial measures? My answer is, that we cannot, inasmuch as each individual case presents its own peculiarities, and the judicious practitioner will look to each for its own indications for treatment. We may state generally, that blood-letting, mercury, purgatives, blisters, darkness, absolute quiet of the part, and of the mind and body, are the chief means for all; any of which, taken separately, may prove quite inadequate for its cure; and that blood-letting is to be regarded not so much a cure of retinitis as a means of allaying its activity, and thus affording a favourable opportunity for the action of mercury—a remedy which can even restore vision when lost from effusion of lymph into the texture of the retina, provided its constitutional influence have been induced before that loss has become habitual. Moreover it does not follow of necessity

* "Lectures;" *Lancet*, vol. i., 1840–41, p. 526.

that local blood-letting is inadmissible, even when the general powers are most depressed, since rapid, intractable, and disorganising local action, is frequently co-existent with such a condition of the general powers. Where such is the case, it is lawful and proper to draw blood locally, by cupping or leeches, whilst at the same time we uphold the general powers, according to circumstances, by diet, tonics, or even stimuli.

Blood-Letting.—In sthenic conditions of the constitution, and in plethoric individuals, when the symptoms are very active and acute, the pulse hard and full, skin very hot, and the headache severe, the patient should be placed in the erect posture, and bled to incipient syncope. In these cases, the greatest possible relief is obtained, of both the local and constitutional symptoms, by the use of the lancet. The necessity for a repetition of the venesection is indicated by the marked relief afforded by the first, and by the violence of the constitutional and local symptoms. Should the hardness of the pulse, the activity of the inflammatory fever, the headache, and the deep-seated and agonising pain and tension of the globe be renewed, we must have recourse to a second general blood-letting. Cases, however, requiring such treatment are rare indeed. Blood-letting alone is quite inadequate to prevent disorganisation, though a powerful means of subduing the painful and severe constitutional and local symptoms. Dr. Jacob says truly, when he observes that daily experience proves how unavailing mere depletion is found in iritis, or general internal inflammation, and even how unsuited to particular cases, however intense the symptoms.

Local depletion, by leeches or the cupping apparatus, should be employed, in general, after general bleeding. In less violent cases, which form the great majority of examples of the acute form of retinitis, or where the powers of the constitution are from any cause deficient, they form the only allowable or necessary means of abstracting blood.

Mr. Tyrrell, as I have remarked before, thinks that the force of vascular action is usually below par in these cases; and he says, that general abstraction of blood is not only unnecessary, but even likely to prove injurious.* It should never be resorted to whilst the pulse is easily compressible, only when it is hard and resistent,

* *Cyclopædia of Practical Surgery*, vol. i., pp. 81–2.

and even then very cautiously. Local bleeding, by the cupping glass or leeches, must also be cautiously employed when the circulation is depressed and feeble, or its effects may be injurious instead of beneficial. Unfortunately, says Mr. Tyrrell, the loss of blood, generally or locally, often mitigates suffering for a time, and therefore induces a patient to solicit the repetition of it, and too often the medical man, ignorant of its baneful tendency, follows up the deceptive plan of treatment. The disease cannot be arrested or cured by loss of blood: the only good which can be effected by it, in Mr. Tyrrell's opinion, is lessening tension of vessels, so as to retard the disorganising process, and to allow time for the operation of other remedies. He would only, therefore, abstract blood generally when the pulse is hard and incompressible, and then only in sufficient quantity to relieve the tension of vessels; but otherwise, he would prefer the use of leeches on the palpebræ, as affording the means of emptying more readily the vessels of the affected part—a practice I should by no means be inclined to adopt myself; and in particular cases, where the leech acts injuriously, he would apply the cupping glass to the temple or behind the ear: to the former spot, if no hemicrania existed; to the latter, if there were any cranial pain or tenderness.

Mercury.—Having had due recourse to blood-letting, we should place the patient (nothing contra-indicating) under the influence of mercury as speedily as possible. It is well to commence its operation as a purgative, for which purpose ten or even twenty grains of calomel may be administered, and followed up, a few hours afterwards, by an ounce of the oleum ricini. As soon as the purgative has ceased acting, the calomel should be introduced into the system so as speedily and effectually to influence the constitution, at the same time to ruffle its powers as little as possible. Calomel and opium, in the proportion of one grain of the former and a quarter of a grain of the latter, should be given every hour. We should effect a decided soreness and tenderness of the gums, a coppery taste in the mouth, and a mercurial fœtor, but go no further. I believe that complete salivation would be much more likely to augment the danger than to remedy the disease, and that we shall do as much good by a decided impression on the salivary glands and gums, as by making the patient stream with saliva. Having once fairly affected the constitution, we should maintain

that influence by suitable doses of the mineral,—about double the quantity first used, every six or eight hours, viz., calomel two grains, opium half a grain,—the dose being diminished or increased, according to existing symptoms, but continued as long as acute symptoms remain. This accords with Mr. Travers' opinion, who says,* that mercury should be introduced into the system with all convenient rapidity, when the amaurosis is recent and sudden. No advantage is obtained by salivation; on the contrary, he thinks it hurtful. When mercury is beneficial its effects are perceived as soon as the mouth is sore.

To give the mercury a fair chance it should be as promptly used, after seeing the patient, as possible. Dr. Copland† justly observes, that much of the advantage to be obtained from this mineral in the acute inflammatory form of amaurosis, as well as in iritis, depends upon the promptitude with which it is employed. Should zonular redness, increased sensibility to light, and blindness, more or less complete, remain after the acute symptoms have been subdued, it is necessary to maintain a slight influence of the mercury for weeks or even months. For this purpose, should the calomel disagree, some milder preparation of mercury may be used, as the pilula hydrargyri with the extract of conium, or the hydrargyrum c̄ cretâ.

Turpentine.—Should mercury be inadmissible from any circumstance, turpentine, which possesses a similar power, in a minor degree, over the capillary vessels, in preventing an effusion of lymph, would be a valuable substitute. I have tried it in several forms of internal inflammation, but think it less efficacious, in acute cases, than mercury, but that it possesses a very valuable efficacy in slow and protracted examples. Dr. Copland states, that the success which has resulted from the exhibition of the oleum terebinthinæ in iritis, induced him to prescribe it, after depletion, in two cases of this form of retinitis, and with satisfactory results in both. In persons far advanced in life, in scrofulous subjects, especially where there is much predisposition to phthisis, and in debilitated persons, this oil is certainly a less hazardous medicine than mercury, exhibited so as to affect the system.

* *Synopsis of the Diseases of the Eye;* second edition, p. 311.

† *Dictionary*, vol. i., p. 50.

Mr. Carmichael* uses the oil in drachm doses, given three times a day, for iritis, which is applicable also in the present case. Its disagreeable flavour and nauseating effects, he has found best obviated by almond emulsion. This circumstance it is very necessary to attend to, the medicine being so unpleasant, that if its taste be not in some way disguised, it is difficult to depend on patients taking it with the necessary regularity. In the formation of the emulsion, if double the quantity of the confection directed in the *London Pharmacopœia* be employed, (that is two ounces to the half-pint of water,) it answers the above objects better: the residuum may be removed by straining.

With an emulsion so made, the following is the formula Mr. Carmichael adopts generally:—℞ Olei terebinth. purif. ʒi.; Vitellum unius ovi; tere simul et adde gradatim; Emulsionis amygdalarum, ʒiv.; Syrup. cort. aurantii, ʒij.; Spirit. lavand. comp., ʒiv.; Olei cinamomi guttas tres vel quatuor. Misce. Sumat cochlearia larga duo ter in die.

In a few cases it has been necessary to increase the quantity of turpentine to an ounce and a half or two ounces, in the above mixture, the other ingredients being proportionally diminished, so that a drachm and a half or two drachms of it may be taken each time; but in general, when administered to the extent directed in this formula, it has very seldom indeed failed, though extensively tried, and in very urgent cases. The strangury, so frequently induced by the internal use of turpentine, is obviated by the usual means—flax-seed tea and camphor julep: when very urgent, the medicine may be suspended for a time. The tendency to acidity on the stomach, which it sometimes causes, is relieved by the addition of the sesqui-carbonate of soda to the mixture,—ten or fifteen grains to the eight ounces will be sufficient. Some patients asserted that the taste was still further disguised by this addition.

When the local inflammation is high, and acute pain is present in the eye and side of the head, Mr. Carmichael recommends the abstraction of blood from the temple by cupping, or from the more immediate seat of the disease by leeching, conjointly with the turpentine, on the same principle as where mercury is used. Nevertheless he has often relied solely on the turpentine mixture, even when the symptoms were very urgent, and with the most

* *Observations on the Efficacy of Turpentine in Iritis*, &c., pp. 9–11.

decided and expeditious relief. Indeed, in some instances, where the pain and hemicrania existed as acutely as they are perhaps at any time to be met with, patients have declared that they were considerably relieved after they had taken it once or twice, and that its subsequent exacerbations were lessened in a very remarkable degree. It is in the former cases he has generally found it necessary to follow up the bleeding by increasing the quantity of the turpentine. It is highly necessary to observe, he remarks, that the condition of the bowels will require attention: the beneficial effects of the medicine appear to be suspended in certain cases, when constipation is present, and are called forth, as it were, when this is removed.

Counter-irritation.—After premising blood-letting to the extent allowable by the circumstances of the case, and when the constitutional symptoms of active fever have been relieved, blisters should be applied behind the ear, or a large one opened at the back of the neck; and, where the disease threatens to become chronic, we may employ the tartar-emetic ointment to the same situation. Blisters may prove injurious if used too near the locality of the disease, or before the acute symptoms have been subdued by depletion. Issues, setons, moxas, the actual and potential cautery, errhines, and sternutatives, as recommended by different authors, are quite inapplicable to acute retinitis. Besides these measures, perfect rest of mind and body, diet suited to the condition of the patient, darkness, and seditive applications to the eye and eyebrow, are valuable auxiliary measures. Dr. Jacob says that the eye should be protected from the irritation of strong light, but that total darkness is unnecessary, and probably pernicious, by increasing the sensibility of the retina. Care should be taken that, in excluding light, the free circulation of fresh air round the patient should not be interrupted. "No circumstance," Dr. Jacob says, "has contributed more to the want of success, in cases of this description, than confinement of the patient to the respiration of an atmosphere contaminated by human effluvia."

Colchicum has been proposed when the patient is either gouty or rheumatic; but, as far as I have observed, it is utterly unworthy of a place in the catalogue of ophthalmic remedies. Under all circumstances, the eyes should be protected by an ample green shade, besides darkening the apartment. Should abscess of the

globe occur, it would be advisable to puncture the cornea, and thus save the patient from the great pain and torment which the distention would occasion, before a natural exit was provided for the matter, by ulceration, splitting, or sloughing of the cornea.

Mr. Middlemore's* opinions and practice seem to accord pretty nearly with my own. He thinks that, as retinitis is so rapid in its progress, and so likely to impair the integrity of the retina, it is highly important that any remedies employed should be equally powerful in their operation, and prompt in the production of their effects. He advises the copious removal of blood, the rapid production of salivation, and the exclusion of light from the patient's apartment, combined with a low unstimulating diet.

A great deal of care and judicious management are necessary in preventing relapses, after the acute symptoms have been subdued. The patient should use easy but regular exercise in a pure air; his diet must be mild and nutritious, and his bowels kept in a gently lax condition; he should carefully avoid all cold and wet, especially wet feet. If any conjunctival redness remain, an astringent collyrium, as two grains of the sulphate of zinc to the ounce of rose water, or dilute vinum opii, should be used two or three times daily. Any return of the symptoms of inflammation should be combatted immediately, by the application of leeches or blistering, and the alimentary canal thoroughly cleared out by an appropriate aperient.

CHRONIC RETINITIS.

As I have had to remark of acute retinitis, so I must now observe of the chronic form, that it presents many grades: it may be active or passive in its symptoms, occupy one or both eyes, and may have degenerated into a chronic affection from the acute, or have presented a similar character from its very commencement.

Symptoms.—On examining an eye affected with chronic retinitis, we find the sclerotic redness, in general, slight. There is, however, a well-formed, although pale, zone of very fine, straight,

* *Treatise*, vol. i., p. 797.

dull-red vessels around the cornea, which we may readily perceive, on a careful inspection, do not reach the cornea. A few varicose vessels are observable on the conjunctiva, and the lids are redder than natural: some authorities represent this tunic as being preternaturally dry. The pupil loses its clear black colour; the pupillary edge of the iris becomes irregular, displaced, and sometimes discoloured; vision is greatly impaired, and the patient complains of various amaurotic symptoms. The field of view is very misty and confused, and not unfrequently distorted and broken. Bright objects appear variously coloured, and surrounded by a halo; and almost all substances seem tipped with rainbow colours. This amaurotic confusion of vision merges insensibly into the diseased sensation of photopsia, of which the patient especially complains. He has a sensation of sparks and flashes of fire before the eye or eyes, or of brilliant metallic globules; sometimes they are of a red colour, and resemble flowers and numerous petals, or falling stars. The patient complains of a dull aching pain in the eyeball and over the brow, varying in severity in different cases, but suffering an aggravation nocturnally, and of frontal headache, which may or may not affect other portions of the head. An increased sensibility to light, also, invariably accompanies the early progress of chronic retinitis, before complete disorganisation and loss of sensibility result. In the early stage, too, the pupil is contracted, and in some cases to an almost incredible extent, so that it is difficult to detect a central aperture at all; but as the retina becomes insensible, the pupil dilates, and becomes quite motionless. The eye is very tender to the touch, and feels, in general, firm and distended, but sometimes, on the contrary, soft and flaccid.

The progress of this disease is usually slow, and in some cases very slow, but its ultimate effects are complete loss of vision, disorganisation of the retina, and sometimes of the surrounding parts, with a great tendency to spread to the opposite eye, and act over again the same symptoms, to affect the same tissues, and disorganise the same portions of those structures. Mr. Middlemore* thinks that it generally advances very slowly, and without much pain, and that the inflammation is usually limited to the retina, the sensibility of which is gradually destroyed by the deposition of

* *Loc. cit.*, p. 798.

lymph within or upon the texture, and it is not improbable that other organic changes also occur. He thinks, also, that this chronic state of disease possesses a tendency to establish a varicose enlargement of the choroid vessels, and of the arteria centralis retinæ and its ramifications.

Case 8.—John Rabjohns, aged 40, was admitted as a patient of the West of England Eye Infirmary, under the care of Mr. Barnes, on the 17th of August, 1838. He stated that he had already suffered from similar symptoms for more than a year. The pupils of both eyes were so much contracted as scarcely to be visible, with discolouration of their pupillary edges. Scarcely any useful degree of sight remained, but luminous appearances were still very troublesome, and the sensibility of the eyes to light was increased to such a morbid extent that its influence was intolerable. A slight degree of pink-coloured sclerotic vascularity surrounded, but fell short of the corneal edges, and the conjunctivæ presented a few varicose branches of a dark purplish hue, which passed inwards from the recti branches. The patient complained of severe occasional headache, and of nocturnal pain over the brow. He was directed two scruples of the pulvis purgans, (Pulveris Sennæ, Pulveris Jalapæ, Potassæ Bitartratis, partes æquales,) a blister to be applied to the back of the neck, poppy fomentation to the eyes, and two grains of the hydrargyri chloridum, with half a grain of opium, in the form of a pill, every night.

August 20.—He was ordered to continue with the same remedies.

August 27.—The mercury had made his gums sore, but he complained much of headache and beating of the temples, and altogether his symptoms were more severe; accordingly twelve ounces of blood were taken from his arm.

August 31.—He had been decidedly improved by the venesection, and all the more acute symptoms were removed. In addition to the remedial means previously prescribed, he was now directed to use the unguentum antimonii potassio-tartratis to the back of the neck.

The eruption from the tartarized antimony ointment, and a slight soreness of the gums was maintained for a considerable time, with but partial success; great contraction of the pupils and intolerance of light still remaining.

December 23.—The dilute vinum opii was prescribed, to be dropped between the lids, which produced evident relief of the conjunctival symptoms.

Notwithstanding the activity of the remedial means employed, country air, and attention to his general health, he continued to attend occasionally during the spring and summer of the year 1839, complaining of great obscurity of vision, intolerance of light, and photopsia, in a diminished degree certainly, but the pupils still remained contracted to the utmost degree. He went on employing remedies of similar kind and remedial action, and was finally discharged at a time when the symptoms were decidedly improved.

Case 9.—Ann Stuckey, aged 30, was admitted at the Exeter Eye Infirmary, as a patient of Mr. Delagarde, on the 5th of October, 1838, affected with chronic retinitis. She complained of great intolerance of light, various and unpleasant sensations of light, headache, and almost total loss of vision. On examining the eyes the pupils were found to be greatly contracted, and there was a faint zone of very fine straight sclerotic vessels around the cornea. She was directed to apply a blister to the back of the neck, to take five grains of the pilula hydrargyri every night, and a pill of five grains of the compound extract of colocynth, with one-eighth of a grain of tartarised antimony every morning.

October 9.—Symptoms much relieved, but the bowels were obstinately confined. To take two of the colocynth pills every morning.

October 16.—Vision much improved. To continue the remedies already prescribed.

October 26.—Still to continue the same remedies.

November 3.—She was ordered to use the tartarized antimony to the back of the neck. Under this treatment the inflammatory symptoms soon subsided, and she entirely recovered the vision of both eyes.

Constitutional Symptoms.—During the progress of the above symptoms the patient complains more or less of disturbed health, anxiety of mind—fearing the result of his ophthalmic disease,

and, in general, a great tendency to a deranged state of the digestive organs, and constipation. The skin is apt to be harsh and dry, and the patient complains of a burning sensation in the palms of the hands and soles of the feet, especially at night. The pulse may be natural, but where the symptoms are of the active type it is harder and fuller than natural.

In passive cases the pulse is soft and quick, there is much debility, and the fever assumes a hectic character.

Causes.—The predisposing causes are nearly similar to those I have mentioned for the acute disease. The most powerful are the previous occurrence of the disease, and the sympathetic influence of one eye on its fellow. Beside these, a low and constrained position of the head during work, with a protracted and habitual exertion of the retina on small objects, tending to produce and keep up a deranged circulation in the tunic.

As regards the exciting causes, the great majority of patients state that it was first brought on, and has since been increased, by their employments, which require much exercise of their eyes on minute objects brilliantly illuminated, or requiring much attention, effort, or even, sometimes, great straining of their sight. Sometimes acute retinitis subsides gradually, and gives place to the symptoms of the chronic variety. I have seen the disease evidently dependent on constitutional derangement; and I will now narrate a case, which, whilst it presents a fair specimen of chronic retinitis, warrants me in stating that it is sometimes a rheumatic affection.

Case 10.—Ann Ash, aged 43, has suffered more or less from the same affection for eleven months. She states that she is a washerwoman, usually enjoying very good health, but is subject to chronic rheumatic affection of various joints; has had a large family, and is of habitually temperate habits. Her irides are light-coloured.

The previous history of her complaint is as follows:—She had been affected with rheumatism of her joints before she discovered anything amiss with her vision; but with the rheumatism her eyes had been painful, and watered from bright light—an affection which had disappeared with the rheumatism. About nine months

since, (from June 5th, 1840,) on shutting one eye, she found herself quite blind with the other, namely, in the right eye. The blindness consisted of simple obscurity of vision, accompanied by rheumatic hemicrania and dyspeptic symptoms, but with little or no pain in the eye itself.

On examination, the pupillary edge of the iris was found discoloured and retracted; the fundus of the globe opaque and buff-coloured, with general haziness of the pupil; there were several large conjunctival vessels, and a pale pink zone of very minute straight vessels around the cornea; the pupil was contracted and sluggish in its movements, when the healthy eye remained uncovered, but, on the contrary, became dilated and motionless as soon as the light was completely excluded from the sound organ. She has decayed teeth, which at present give no pain.

She complained of deep-seated and aching pain in the affected (right) eye, invariably aggravated at night; severe frontal headache; pain over the brow, and "beating in the temples." The character of the pain was dull and oppressive, and rendered her incapable of much bodily or mental exertion. She states that, although all useful vision is lost in that eye, she fancies that she can see better in a weak light, and is frequently troubled with the appearance of small dark objects moving about before the eyes, and sparks, and flashes of light.

She is affected with pyrosis, coming on after breakfast, and thus frequently brings up a large quantity of a clear and, to her taste, acid liquor; the appetite is good; the functions of the large intestines regular; the catamenia normal in quantity, duration, and regularity; the skin, especially of the palms of the hands and soles of the feet, becomes hot and dry at night, with thirst; the tongue clean, but swollen, and flabby and marked with the teeth; pulse 92, full, but readily compressible. Her health, before she became affected with rheumatism, was very good, but she was always "very nervous."

The diagnosis from these symptoms was chronic retinitis, (rheumatic,) with cerebral congestion; blindness from effusion of lymph into the texture of the retina.

This ophthalmic disease had existed more or less for eleven months. She applied in October, 1839, about two months from

the commencement of her symptoms, and was then treated by blue pill every other night, a pill of colocynth and the potassio-tartrate of antimony every morning, and the tartar emetic ointment to the nape of the neck. On the 16th of December, a blister was opened at the back of the neck. This treatment was continued, with scarcely any modification, until March, 1840, when she was discharged cured; having declared that she had quite recovered her sight, and lost all unpleasant symptoms. In June, 1840, she again applied with the symptoms, and in the condition I have described. Purging, counter-irritation, and mercury, were had immediate recourse to. The mercury was used in the doses, and with the views I shall presently explain, namely, a very slight but continued constitutional action, as denoted by slight soreness and redness of the gums. This treatment again proved successful; the progress of the disease was arrested, and the disorganisation already effected, by the continuance of diseased action, removed. She was discharged cured, in October, 1840.

Remarks.—This case, again, proves that blindness from recent effusion of lymph into the structure of the retina, is often curable by a discreet employment of mercury; and that, if it possessed any title to the name of rheumatic, it was as curable by mercury and other anti-phlogistic measures, as retinitis, presenting nothing uncommon in its origin, progress, and causes. Should we state that there is such an affection as rheumatic retinitis?—that the retina may be inflamed, as one of the manifestations of a constitutional disease? The foregoing case would, in my estimation, prove as much, namely, that the retina may be inflamed during the progress of rheumatism, and be attended with local and general symptoms peculiar to that affection. Nevertheless, it seems to me to be of little moment whether we regard such as rheumatic, or simply as a complication, since the indications for treatment are similar in both instances. If the sclerotic coat were simultaneously inflamed, and long resisted the influence of mercury, I should be inclined to try the effects of the iodide of potassium, as it possesses the most undoubted power in chronic inflammation of the joints and fibrous tissues; and different structures are differently influenced by the same remedy, whilst analogous tissues are amenable to the same laws. Lastly, the present case especially directs our attention to the necessity of great caution from relapses,

since the patient was discharged, apparently quite well, in March, and returned, with the retina opaque from effusion of lymph, and with almost total destruction of vision, in June. The disease had doubtless been slowly creeping on in the interval. The retina was left prematurely exposed to exciting causes, and the disease had relapsed, most probably from exposure to damp and cold. Hence we may come to the conclusion that the patient is not safe, not cured, until this predisposition itself is removed also. To effect this, all the exciting causes should be most rigorously avoided, and counter-irritation be kept up for some time in the vicinity of the suffering organ.

Diagnosis.—Intolerance of light occurs in the early stages of some amaurotic cases, independently of inflammation, and also, in a very severe degree, in strumous ophthalmia; so that it cannot be supposed to indicate a chronic degree of retinitis, when uncombined with the other peculiar symptoms of that disease. Strumous ophthalmia occurs chiefly in children: vision, in these cases, is uninjured, for when the excessive photophobia is relieved, and in the twilight or a darkened room, they see perfectly; pustules are generally present on the edge or surface of the cornea, and the vascularity is of the fascicular character. In chronic retinitis, vision remains imperfect, or entirely lost, in diminished light, although the intolerance is relieved, and the spasmodic contraction of the orbicular muscle ceases. Instead of a bundle of vessels tapering to one point, a circle of very fine, straight vessels, of a claret colour, form a halo around the cornea, but do not actually reach its edge. As Dr. Mackenzie has remarked, the patient seeks the dark, but there he keeps the eyes unshaded and open; whereas the eyelids are kept shut, even in the dark, during the paroxysm of strumous photophobia.

In chronic retinitis, the patient is troubled with luminous spectra; deep-seated pain in the eyeball; a distorted, broken image; pain over the brow, with its nocturnal aggravations; and, generally, nocturnal febrile excitement. One of the most characteristic symptoms of struma is, a peculiar form of dyspepsia, which the late Dr. Todd* described under the name of "strumous dyspepsia." In Dr. Todd's opinion, it presents a more characteristic

* *Cyclopædia of Practical Medicine*, vol. ii., p. 654.

feature of this habit of body than any physiognomical portrait which has yet been drawn of it. In this respect, it is more to be depended on than either the fine skin, the clear delicate complexion, the light hair, large blue eyes, and dull sclerotica, of one variety; or the foul, dull, swarthy-coloured skin, the sallow complexion, and swollen countenance, the dark hair, and tumid upper lip, of the other. It betokens, indeed, little familiarity with scrofula, to connect it with any particular temperament; for it belongs to all temperaments; to the sanguine, as well as the phlegmatic; to the nervous, as well as the melancholic; and to all their varieties and combinations. But on whatever temperament the disordered habit, which we call scrofula, may engraft itself, we venture to say that this form of dyspepsia will also there be found, and accompanying the various symptoms which issue from it. It would, therefore, be contrary to all reason to refuse to it an important share in the development of this disordered habit, and in the production of the local affections, which have hitherto too much engrossed the attention, to the exclusion of a proper consideration of the constitutional disease.

Prognosis.—Although there is not that immediate danger which exists in the acute forms, still the unerring termination of the disease is to complete loss of vision, and to the destruction, also, of the fellow eye, if the disease be not controlled, kept under, and watched for some considerable time, and if the patient does not, to the best of his power, assist the practitioner, by the careful avoidance of exciting and predisposing causes. Where, however, we can command the best efforts of the patient, and have the opportunity of conducting a mild, steady, and efficient treatment, for some time, watching its effects, and acting accordingly, the result is almost sure to be favourable, provided we have seen the patient sufficiently early, and there be nothing very unfavourable in his constitution. Relapses are more unfavourable, but the same remarks apply to their probable terminations.

Treatment.—It is rarely necessary to employ general bloodletting; nevertheless, should the symptoms be severe and active, the individual robust and plethoric, with determination of blood to the head, it would be had recourse to with benefit. Local abstrac-

tion of blood, by cupping or leeches, may generally be used with advantage, but are inapplicable where the case is of passive type. When applicable, it must be used in proportion to the severity and activity of the symptoms, and the constitutional vigour and powers of the patient.

The same rules apply in the selection of mercury and turpentine as in acute retinitis, but the mode of using mercury is different in the chronic forms. Here we cannot at once put a stop to the diseased actions by a powerful impression on the constitution, but we must follow, as it were, the course of the disease itself, and go calmly, steadily, and slowly to work. We employ mercury so as to effect the mildest possible constitutional influence, with slight soreness of the mouth, and mercurial fœtor. Calomel and opium, or blue pill and conium, are best fitted as the form for exhibiting the remedy: they should be employed in moderate doses, at stated and regular intervals, the quantity being decreased as soon as the gums indicate just that extent of inflammation which shall interfere least with the mouth's ordinary functions, and continued in diminished doses, so as to maintain permanently this condition. It must be continued rather longer than the apparent disappearance of unfavourable symptoms, on the grounds which I have already mentioned, namely, the fear of relapse from the vessels of the retina remaining dilated. The constitutional powers will thus be restored unbroken, even though we maintain its action for many months; whilst the inflammation and its effects will as surely yield as if we added the dangers and inconveniences of profuse salivation. This is the way in which almost all chronic inflammations are cured by mercury, most safely, most quickly, most pleasantly, and, I may add, most permanently.

It is of the greatest importance to maintain some permanent source of counter-irritation in the neighbourhood of the disease; such as a succession of blisters behind the ear or to the back of the neck, or kept discharging by savine cerate, &c. In many cases, the best mode of producing and keeping up a brisk eruption and discharge is, by the application of the ointment of the potassio-tartrate of antimony to the back of the neck, and, in some chronic cases, the use of the seton.

The exciting causes must especially be avoided; the eyes should be shaded, and the most absolute and perfect rest of the organs

employed, carefully avoiding the inspection of everything requiring careful vision; but passive exercise, as in viewing the surrounding country during walking, riding, &c., may be beneficial. The greatest possible attention should be paid to the general health; such as by regular exercise, (to the avoidance of fatigue,) country air, regular, unirritating, mild diet; the removal of any existing source of irritation; attention to the state of the bowels, which should be kept gently lax by the use of occasional mild purgatives; the warm bath, when the skin is rough and dry; warm clothing, &c.

There is a still more chronic form of retinitis, which I will but briefly sketch. Patients occasionally present themselves, in whom we trace the symptoms of a very chronic form of retinal inflammation; more, in fact, those of retinal hyperæmia than inflammation, being very slow in their progress, and ultimately inducing changes different from the usual effects of chronic retinitis. This form of disease is scarcely, if ever, spoken of as retinitis, but placed under the indefinite term of "amaurosis," which, like many other terms, serves as a convenient cloak for ignorance or sloth. It is not by any means a very common disease *per se*, but comparatively so, in common with active or passive cerebral congestion. In this variety of retinitis the eye has a dull congested appearance, the conjunctival and sclerotic vessels are enlarged, vision fails gradually, attended with luminous appearances before the eyes, muscæ volitantes, a distorted broken image in the perception of objects, &c., whilst a gradual change of colour is effected in the pupil, which becomes misty, and presents a turbid, somewhat greenish hue. Here there is no stage of intolerance of light, but from the beginning diminished sensibility of the retina, and desire for bright light. In proportion as the blindness increases so the pupil dilates, and moves with a sluggish and very imperfect motion. In general hyperæmia of the visual textures, both eyes are simultaneously affected; in this chronic form of retinitis, usually but one: but after the continuance of disease for some time, its fellow frequently suffers from a similar disease of the same character and parts.

All the usually described symptoms of amaurosis may be present in this disease, or any or most of them be absent. Although very

chronic and intractable, frequently terminating in incurable disorganisation, yet, when actively and timely treated by an antiphlogistic and mercurial plan, many are brought back to the precincts of health. Mercury, unless very early and steadily used, is less effective than in the more acute cases, since the very actions which disorganise, render these conditions permanent during the continuance of the disease itself.

AN ESSAY,

LITERARY AND PRACTICAL,

ON

INVERSIO UTERI.

BY JOHN GREEN CROSSE,

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Having been led to select a subject, upon which no individual can have much experience, I shall feel little hesitation about availing myself, to the utmost extent that my limited time and opportunities will permit, of the labours of preceding writers. To skim over the surface of medical literature, and quote one author upon the authority of another, is not unusual, as can be proved by abundant evidence; but I will not allow myself to appear learned through this manœuvre, and will quote only such authorities as have come under my own inspection, referring generally to the sources whence have been selected whatever seemed most suited to the occasion. In thus mixing the gems of the best writers with my own crude and ill-adjusted language, I claim a favourable consideration, which all, save the hypercritical by profession, will willingly concede. To combine voluminous meaning with brevity of sentence, is a task rarely accomplished even by the practised writer. The reviewer is generally diffuse in style, the novice abrupt and obscure. After looking through so many books, I have imbibed a strong desire to avoid filling a single page with mere verbiage, bereft of fact, argument, or inference; so I will proceed, *coute-qui-coute*, to develope, after my own manner, the subject of my laborious research for several months past.

SECTION I.

HISTORICAL REMARKS AND TERMOLOGY.

There can be no doubt that inversion of the womb has occurred in all ages, since the world was fully populated;[1] but only in comparatively recent periods has it been separately recognized and described. In the earliest existing records, this displacement is constantly confounded with direct descent, or prolapse, of the womb. Hippocrates is so obscure in those remarks which can alone be regarded as referring to this subject, that it is not possible to determine when he refers to inversion, and when to prolapse independent of inversion; his most forcible sentence is, that in the greatest prolapse,[2] "the uterus hangs down from the *pudenda* like a scrotum." Aretæus goes further, and states that "the disease

[1] "Il faut convenir que dans ces temps reculés, cette accident devait être plus rare que depuis qu'il existe des accoucheurs, et des sages-femmes, et surtout depuis qu'ils se sont autant multipliés. A peine trouve-t-on quelques exemples dans les auteurs anciens, tandis que les modernes en présentent, pour ainsi dire, à chaque page; et que le nombre en paraîtrait immense, s'ils s'etaient donné la peine de les recueiller tous."—Dailliez, p. 1.—Unless it be admitted that a more civilized state of society has a deleterious influence, and that less good is done in proportion as more knowledge is possessed, we must controvert this doctrine, and rather attribute the rarity of known instances in early times to the want of observation, the incorrectness of the diagnosis, and the paucity of channels for publicity. In free countries, much more is heard of *miseries* where the fewest exist; in barbarous or despotic countries *they* abound, but are unspoken of, and pass away silently: so it is with diseases. Dr. Meigs writes, "That he lived for some time in early life among the Cherokees, and has seen the Ollowas at Makaia, and the Chippewas at the Saulte de Sante Marie, and is led to remark, that people so barbarous must have an obstetricy scarcely superior to that of the lower mammals, and that from the nature of things they ought to be more frequent subjects of inversio uteri than civilized people; and he suspects that a *Polowallamie Lancet*, or a *Blackfoot Medical Gazette*, would furnish as many cases of obstetric accidents and fatalities as the London, German, or Paris journals." Amongst the most uncivilized, there are females who officiate as midwives; and a correspondent, well known to me, (Dr. Arnold, of Kingston, in Jamaica,) after relating two cases of uterine inversion, *post partum*, in negresses, one ending fatally, adds—"Such cases are by no means uncommon, arising, for the most part, from the officious and ignorant meddling of the women in all parts of the island, who act as *grandys*, or midwives." The subject might be pursued further; but it will suffice, if we take care to educate *our* midwives better, and abuse them less.

"Ην δὲ παντάπασιν ἐκ τῶν ἀιδοίων ἐκ πέσωσιν ἐκκρέμανται ὥσπερ ὄσχη."—*Opera a Vander Linden*, ii., 361.

generally proves fatal, and may arise in a difficult delivery, in which the adhering after-birth is forcibly extracted, and with it the coats of the uterus;"[3] language so strictly applicable to the displacement in question, that it seems extraordinary to find Haller,[4] and other able commentators, expressing an opinion that Aretæus spoke only of uterine prolapse. When Celsus[5] speaks of "a fleshy mass, descending at the labia of a certain female, and causing death in a few hours, neither the nature of the disorder nor any appropriate remedy being known," it is scarcely possible to avoid concluding, with Morgagni,[6] that an inverted uterus was the cause of death: a polypus suddenly protruding, as has been suggested,[7] would not explain the case so well, nor would it prove fatal so quickly unless it inverted the womb.

Moschion,[8] the first author of a separate treatise on midwifery, removed none of the obscurity left by his predecessors; and Galen[9] kept to his grand prototype, likening the external prolapse to "the scrotum containing the testes;" adding, however, a striking comparison, which could only be the result of his meeting with a sudden inversion of the womb immediately on delivery: he had as surely met with the womb inverted and prolapsed, as he had seen wrestlers at their public games. Paulus Ægineta[10] deserves to be referred to: he mentions rupture of the ligaments of the womb as leading to its descent or prolapse. No advance was made by Ætius; and, after many centuries, we find Albucasis, in the chapter

[3] "Hæc verò abscedit et fluxione, et abortu, et violento partu, quando ipsa secundis inhærescit; nam quum ipsæ vi extrahuntur, simul et uteri tunica extrahitur."—p. 65.

[4] Peyrilhe *Histoire de la Chirurgie*, ii., 218.

[5] "Ætate nostra quædam, ex naturalibus partibus carne prolapsa et arente, inter paucas horas expiraverit."—*Præf.*, p. 13.

[6] *De Causis et Sedibus Morborum*, ii., ep. 45, p. 393.

[7] Dailliez, p. 6.

[8] "Matricis casus, (vel ejus procidentia,) fit cum infantem mortuum embryulcis adducimus, vel obstetrices imperitæ cum secundinas indirectum adducere conantur."—p. 36.

[9] Speaking of the expulsive powers at labour—"Non paucis quoque mulierum, quæ utique immodicè hujusmodi facultate sunt usæ, violenti dolores totum uterum excidere subegerunt, simili quodam in his accidente ei, quod *in lucta et contentione sæpe visitur, qui alteros evertere dejicereque nituntur, ipsi una concidunt:* ita namque uterum, dum fœtum extrudit, ipse aliquando simul excidit, potissimum cum ejus ad spinam *ligamenta naturaliter sunt laxa.*"—*De Naturalibus Facultatibus, Lib.* iii., 303.

[10] "Ruptis pelliculis quæ ipsum viscus continent."—p. 488.

"De Exitu Matricis," still confounding descent and inversion, yet giving the valuable advice, to return the displaced organ[11] "*cum suavitate, non cum violentia.*"

Nothing more distinct than what I have selected, occurs until the time of Ambrose Paré, who flourished in the middle of the sixteenth century, and to whose genius and practical skill we owe the first clear description of inversion of the womb, distinguished from the direct descent of that organ. In the chapter,[12] "De Uteri Procidentia et Perversione," he observes, "In propriam cervicem abscedat, vel etiam cervicem ipsam excedat, et extra vulvam magna sui parte promineat..... Id interdum contingit ex vehementiore pariendi conatu, quo uterus fœtum secundasque excludens impetu consequiter, *seque invertens præcipitat.* Interdum et imprudentis obstetricis temeritas, cum uterum cum fœtu, aut cum secundis tenacius utero obhærentibus, invertit, detrahitque."

Rossetus, who was cotemporary with Ambrose Paré, and Guillemeau, his pupil, and the translator of his works into Latin, contributed still further to make known the causes, diagnosis, effects, and treatment of inversio uteri; and thenceforth the subject became regularly introduced into systematic treatises on midwifery, and cases occurring to each eminent author were not unfrequently recorded.

In the seventeenth century, Hildanus, Bartholin, and Paul Portal, were amongst the ablest contributors; but a discussion arose, which retarded the progress of the subject, and which claims a passing notice. Authors who, even at the present day, are referred to as respectable on other accounts, asserted boldly that the uterus *never prolapsed,* and the contest was warmly maintained by numerous writers on either side. Job à Meek'ren,[13] in a chapter, "De Procidentia Uteri Falsâ," goes on in the following strain:—"Quam ergo pœnam non incurret ille, qui sententiæ antiquorum quæ uterum muliebrem non tantum extra corpus prolabi, sed et in totum auferri posse docet, falsitas declaretur; præstare multis forsan videbitur ab incepto desistere, quam clarissimorum virorum turbæ adeò insigni, procidentiæ extirpationivè uterinæ possibilitatem probanti, sese opponere. At vero non est

[11] *Liber Theoricæ necnon Practicæ Alsaharavii,* 1519, p. 100.

[12] *Lib.* xiv., cap. 40.

[13] Cap. liv., p. 238.

quod me deterreant hæc similiaque, quin contra animosus, anatomico experientiæ cultro armatus, quæ in destructionem figmenti dicti adferri poterunt, proponere aggrediar.

"'Amicus Socrates, amicus Plato, *sed magis amica Veritas!*'"

He then proves, (which writers of all periods could always do, at least to *their own* satisfaction,) the truth of his preconceived notions, referring to actual dissections, and placing great weight upon an adversary's case, where the doctors could not agree,—one thinking it uterine prolapse, and perhaps inversion, others an excrescence from the vagina. "Mulier interim secari volens (nolens?) *inter ambigua consilia ad mortem properavit!*" The uterus was found *in situ*, the tumour proceeding from the vagina. Diemerbroeck[14] treated the *anti-prolapsians* with great and merited severity. Although the reminiscence of such discussions is more amusing than useful, it must not be forgotten that, at the close of this century, so much importance was still attached to them, that Saviard[15] attempted to put an end to the controversy; and the English translator of his *Surgical Observations* sought to recommend the book by announcing, in the title-page, that it treated of "Descent of the Womb, clearly proving the reality of the disease, in opposition to Verduc." The descriptions of Ruysch, given in the most elegant style, and in the Latin language, diffused correct notions upon prolapse, as well as inversion, of the womb, throughout Europe, and made the frequent occurrence of the latter displacement practically known and appreciated. Slevogt

[14] "Verum resipiscat, et palinodiam canat, nam quia nunquam vidit uterum prolapsum, satis temerè et nimis petulanter irridet alios istius rei oculatos testes, ac præstantissimos medicos, illo ipso fortassis longè peritiores. Non fuerunt cucurbitina capita, nec oculi tanto glaucomate obfuscati, ut uterum prolapsum non cognoscerent."—*Opera*, i., 198.

[15] *Recueil d'Obs. Chir.*, Paris, 1702; and Sparrow's Translation of the same Work, London, 1740. But there is no accounting for what even practical and experienced writers will occasionally affirm. At a comparatively recent date, A. Petit asserted that what authors had described as inversion was only descent of the organ—"C'est un être de raison, et tout ce que les auteurs ont pris pour pareil renversement n'est que la simple descente."—*Mal. des Femmes*, ii., 118.—To shield A. Petit, some of his countrymen have suggested that this passage was an error on the part of his pupils, who edited his lectures; but the whole account bears out the same unwarrantable scepticism, and proves that it is one of those bold assertions which very practical but non-literary men too often make, denying whatever they have not themselves seen.

demonstrated the production of uterine inversion from polypus attached to its interior; a division of the subject which was soon afterwards further illustrated by Von Sanden and Vater, as will appear in the practical part of this essay.

Since I am not writing a *Bibliotheca,* I can only briefly refer to the original writers during the first half of the eighteenth century. Chapman and Giffard gave each a case in this country; also, in France, Pierre Amand, La Motte, Mauriceau, Viardel, and Levret, were contributors from their own extensive experience. Heister gave a good account of the subject in his *Institutiones Chirurgicæ,* which soon became generally diffused, by the translation of this invaluable book into the principal modern tongues. In the second half of this century, several important theses were produced at the greatest schools of medicine; Smellie, Cleghorn, Hamilton, and Denman, showed their acquaintance with the subject, and gave fresh instances of the inverted uterus occurring in this country. The names of Dionis, Deleurye, Astruc, Herbiniaux, Sabatier, are conspicuous as authors in the French language. The *great* Saxtorph, as his countrymen denominated him, gave numerous cases. But the author who most cultivated the subject, and accumulated, during his long and eminent career, the most extensive experience, was Baudelocque: he treated of it at great length in his lectures, stimulated the attention of his pupils towards it, and shed an influence over the treatment, which has continued up to the present day.

The publication of Baudelocque's *Lectures upon Inversio Uteri,* by his pupil Dailliez, in 1803, may be regarded therefore as a fresh era in the history I am rapidly sketching. These lectures contained a vast proportion of what had transpired up to that date, with the particular views of the writer's able instructor. In the following year appeared an excellent monograph by Fries, with plates and plans for the treatment, with a view to reduce the inverted organ. If we exclude numerous theses,—some of them valuable, and containing original cases,—there is no other monograph till our talented countryman Newnham published, in 1817, his *Essay on the Symptoms, Causes, and Treatment of Inversio Uteri,* with a history of the successful extirpation of that organ in the chronic stage of the disease; a work so excellent that it would preclude the necessity of a fresh undertaking, were it not for the

great supply of new materials, which have since been presented through various and scattered channels, by far too numerous to specify, and which must be sought in a systematic list of authors, or will be brought under notice in the following pages.

In surveying the writings of different periods and countries, one perceives the necessity of adverting to *termology*, or the words employed in reference to the subject under consideration. Without precision of language, there can be no accuracy in applying quotations, nor can any definite knowledge be communicated. Such a diversity of terms has been applied to inversio uteri, that the author who investigates for himself, requires to be guided more by the descriptions given of the disease, and the causes assigned for its production, than the name by which it is designated. The same word is applied successively to different diseases;[16] a more fruitful source of error is, the confusion arising from two or more distinct maladies receiving one and the same appellation.[17] A plurality of terms in a particular language, however convenient in composition, impedes the student, and renders an easy matter

[16] In the Latin version of *Albucasis*, the chapter "De Inversione Matricis," is upon quite a different, and not very intelligible malady; and inversion of the womb is considered, so far as it is at all referred to, in the chapter "De Exitu Matricis."—Sennertus (iii., 520) applied *inversio* to a particular hernia of the uterus, where it is displaced through a preternatural aperture.—Puzos called a falling forward of the impregnated womb, till it came upon the thighs with its fundus downwards and its os highest, an *inversion* of the organ. (*Le Blanc*, i., 367.)—Boër much more recently used *inversio* for *obliquity* of the womb. (*Nat. Med. Obstet.*, p. 5.)

[17] *Umkehrung* is the German word long used for *inversio* uteri. Fries and Herzog regard it as the more proper word, the one which I propose to retain, *Umstülpung*, and which is now commonly employed, being of more recent introduction. But Richter (*Chir. Bibl.* iii., 615; iv., 236, 557,) mistakingly applies *umkehrung* to *retroversio uteri*, the well known displacement first described by William Hunter. One of the most noted cases of retroversio is Wilmer's, published in his *Surgical Observations*, in 1779, p. 144, which proved fatal; and after death it was found impracticable to reduce the uterus until the symphisis pubis had been divided. Richter (*l. c.*, 584,) introduced this to his countrymen as a case of inverted womb, and thus it has been quoted by one author after another, without any comment or detection of the blunder! How will our German brethren purge their medical literature of this gross error, perpetuated as it has been by some of their most accredited writers, Fries, Meissner, Kilian, and numerous others? John Lynn's case of retroversion (called *inversion*) of the uterus, (*Med. Obs. and Inquiries*, iv., 388,) and Squire's similar case, (*London Medical Review*, vi., 229,) both fatal, by rupture of the urinary bladder, belong to the same category of misnomer.

difficult. It might interest the philologist, to see a list of these incongruities, and a few of the consequent mistakes, but it would be wholly unsuited to the general reader, and inexcusable if forced upon his perusal by appearing in the text.[18] In concluding this section, therefore, I shall only remark that it would be advantageous, if the displacement, designated in English, *inversion* of the womb, were exclusively signified by *inversio, renversement, umstülpung, rovesciamento,* in the respective languages to which these words belong.

SECTION II.

ON INVERSION OF THE UTERUS AFTER PARTURITION, AND ITS DIFFERENT DEGREES.

In this displacement, (one of the most formidable that can occur,) there is not simply a change as to the place occupied by the uterus, but also a change in the relation of one part of the organ to the rest,—a change of shape as well as of local situation. It can only happen when the uterus is distended, so that its cavity becomes more or less enlarged. When thus enlarged, from whatever cause, the uterus is concave on its inner surface, and convex exteriorly; if any portion of the uterine walls, in its entire thickness, become convex towards the cavity of the organ, that portion is said to be inverted.

Inversion signifies not only a turning inwards of the uterine walls, but a turning inside out of the whole organ, by its passing

[18] Excluding all terms applied to different degrees of inversion of the womb, I find the following amongst the general terms used by numerous writers whom I have dived into:—Greek Compounds: Anastrophe Uteri, (Blasius, *Handb. der Chir.*); Epistrophe Uteri, (Siebenhaar, *Med. Worterb.*); Hysteroptosis, (Sauvage, *Nosolog.*; Busch and Moser, *Handb. der Geburtshulfe*); Hystærocele, (*Herbiniaux*); Metranastrophe, (Rust, *Handb. der Chir.*)—Latin: Eversio, (Guillemeau); Introversio uteri, Perversio uteri, (A. Paré, Desormeaux); Subversio uteri, (Boër.)—French: L'éversion de la matrice, (Paul Portal); Chute ou déplacement de la matrice avec renversement—inversion—l'introversion utérine, (Thouret); Introversion, (Moreau); Retournement, (Guillemeau, Ferrand-Demissols); Perversion, (Peu, La Motte, Amand); Renversement—dédoublement, (Martin-le-jeune.)—German: Involution des Uterus nach dem puerperium, (Rokitansky); Umbeugung, (Herzog); Umdrehung, umkehrung, die mutterumkehrung, (Rust); Umstülpung, die mutter umstülpung, Umsturz der Gebährmutter, (Von Herder.)

successively through the os tincæ, converting the lining mucous membrane into an exterior covering of the uterus, and creating a new cavity, which is lined with peritoneum, and communicates superiorly with the cavity of the abdomen.

It is necessary to distinguish inversion from descent[19] of the uterus, although every preceding writer has confounded the two together, more or less. Descent refers to change of position downwards of the whole organ; prolapse is applied to the uterus, when it descends so low as to pass beyond the *labia externa*, inverted or not inverted.

Pregnancy, the only natural source of enlargement of the uterus, is by far the most frequent of all causes disposing to inversion; and the description given in this section will have reference exclusively to that cause, unless it be otherwise specified.

Inversion of the uterus is either partial or total; the latter can exist only in one degree, and admits of no sub-divisions. Partial inversion, on the contrary, comprises very many degrees; and there are both physiological and practical reasons for noticing and describing three, by way of classification, namely, *depression*, *introversion*, and *perversion*.

Depression,[20] the first division and slightest degree of partial inversion, is present when any portion of the entire thickness of the walls of the uterus becomes convex towards its cavity or interior, without going to the extent of being invaginated, or brought within the grasp of the rest of the uterus, supposing it to contract by the action of its muscular coats. The interior convexity is answered by a concavity of the same extent on the exterior surface of the womb. The posterior,[21] lateral,[22] or even, perhaps,

[19] Descent and procidentia are applied to a displacement downwards of the uterus *en masse;* prolapse is restricted to its precipitation at the labia, so as to be pendulous and apparent externally.

[20] Synonymes.—Eindruck; Depressio fundi uteri; Demi-renversement—la chute du fond de la matrice.

[21] Baudelocque gives a case where the placenta adhered to the upper and posterior part of the uterus, which became inverted. (Bollet, *Thèse*, p. 15.)—Martin-le-jeune met with an instance in his own practice, where the inversion seemed to be formed by the posterior part of the womb: "Par le dédoublement de la paroi postérieure de l'utérus."

[22] "Inversio directa frequentior est laterali, quæ vix nisi à polypo seu excrescentiâ quâcumque, oriri potest." (Deleurye, *Theses*, p. 9.)—Dr. Ingleby has described a

the anterior,[23] part of the body of the organ, may be thus displaced after parturition: but usually the fundus is the part affected, as indicated in the marginal outline, Fig. 1,[24] where (*a*) points to the concavity of the inverted portion, covered with peritoneum, and which has been compared to the form usually impressed on the bottom of a glass bottle,[25] or to the crown of a hat[26] depressed by the fist; (*b*) the cavity of the uterus, encroached upon by the convex surface of the inverted fundus; (*c*) the vagina, its boundary marked by the dotted line at the os uteri; and (*dd*) the angle of inflexion, forming a circular margin around the depressed portion. The palpable evidence of this degree of

variety of inversion in the course of the longitudinal fibres of the uterus: it occupied apparently nearly the whole length of its right side, the left not being sensibly depressed. (*Facts and Cases in Obstetric Medicine*, p. 223.)—Weber recognises lateral inversion. Speaking of the convex tumour, felt on examination *per vaginam*, he says,—"Si fundum representat, in oris centro percipitur, sin est corporis pars lateralis, intervallum in hoc vel illo latere intercedit." (*Dissert.*, p. 20.)—Professor Simpson, of Edinburgh, informs me that he has often met with instances of partial lateral indentations, (scarcely inversions,) attended by severer symptoms than even the hæmorrhage would account for.—My friend, Dr. Ashburner, has supplied the following history:—"A lady, whose labours were always uncommonly urgent, and of short duration, the pains being quick in succession to each other, was in this way delivered of her third child, when in her 28th year.. The last eight pains were almost continuous, like the pains attendant on the exhibition of the *secale cornutum;* the placenta followed the child in less than three minutes; an irregular contraction of the uterus succeeded; the right angle of the fundus was not to be felt through the abdominal parietes; hæmorrhage commenced, and I introduced my hand into the uterine cavity, pushing up a soft convex portion of the uterus, which easily retreated, restoring the cavity. The patient recovered."—Every attentive practitioner of midwifery will meet with such cases, and treat hæmorrhage, after delivery, the more successfully by detecting them.

[23] E. E. Von Siebold once found the anterior part of the uterus, above the os, inverted, in consequence of preternatural adhesion of the fœtal membranes.—*Handb. der Frauenzimmerkrankheiten*, ii., 376.

[24] The letters used in Fig. 1, and repeated in outline Figs. 2, 3, and 4, refer to the same parts in each instance.

[25] Mauriceau; *Mal. des Femmes Grosses*, ii., 186.

[26] Boyer; *Mal. Chir.*, x., 486.

inversion is obtained by examination through the parietes of the abdomen,[27] or by the hand in utero meeting with a convexity of more or less firmness, according to the atonic or contracted state of the part, and giving the idea of a placenta still remaining, after the entire placenta has been removed. If the placenta be still adherent, its attachment is to the inverted portion, and any attempt to remove it by traction will increase the inversion. The leading symptom, after the placenta is detached, is hæmorrhage, of which, as asserted by some writers,[28] it is a more frequent source than has been suspected. The hæmorrhage may prove fatal; Dewees[29] describes a case to this effect; and Mauriceau,[30] in the instance of a woman, twenty-five years of age, who died an hour and half after delivery, from great loss, syncope, and convulsion, found on autopsy only the fundus slightly depressed into the body of the uterus. Simple depression may be unattended by pain,[31] and does not usually persist long, even should it not be detected and appropriately treated; for either the organ recovers itself, or, under the expulsive efforts it induces, and other assisting causes, it passes into the second degree,—*introversion.*

Introversion[32] is when so great a part of the fundus is displaced as that it comes within the grasp of the portion of the uterus into which it is received. The greatest degree of this displacement consists in the fundus and continuous part of the body of the uterus being received into the remainder of its body and cervix,

[27] "Lorsqu'il n'y a qu'une simple dépression ou un enfoncement des parois de la matrice, la tumeur formée par ce viscère présente vers l'hypogastre une espèce de cul-de-lampe dont la main peut apprécier la profondeur à moins que la femme ne soit très-grasse ou hydropique. Le bord de cette fosse est horizontal, ou incliné en arrière, en avant, à droite, ou à gauche, suivant la partie des parois de la matrice qui est enfoncée et la direction de ce viscère."—*Ibid*, x., 495.

[28] See Deleurye, in Note 31.

[29] *System of Midwifery*, eighth edition; foot note to Case 1.

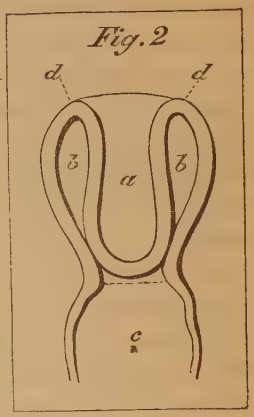
[30] *Maladies des Femmes Grosses*, ii., 182. Fries (p. 59) relates, that on dissection of a woman suddenly deceased after delivery, his father found depression of the fundus uteri, and the bowels filling the cavity, which could only be seen on their removal.

[31] See Dailliez, *Thése*, p. 77.—Deleurye, (*Traité des Accouchemens*, p. 442,) says—"Quand la matrice est renversée dans une petite partie, il n'y a point de douleur; la perte seulement existe, et c'est ce renversement partiel qui en est la cause la plus ordinaire après la délivrance."

[32] Synonymes.—Intussusceptio uterina; die mutter einschiebung; einsackung; ineinanderschiebung der mutter; intusversion; l'introversion uterine.

the convexity of the fundus being palpable at the os tincæ, as in marginal Fig. 2. The inverted portion is in a situation to be resisted, supported, compressed, and otherwise acted upon, by the uninverted, by which it is always surrounded. In proportion to the degree of introversion, is the depth and extent of the peritoneal pouch opening towards the abdomen; the uterine ligaments are in part drawn into this pouch and the ovaria approximate the marginal circle corresponding to the angle of inflexion. On examining above the pubes, the circular margin of the uterus can be felt forming the boundary of the inversion, and the uterus and part of the body of the organ are wanting; in a thin patient, and where the abdominal parietes are relaxed and yielding, the fingers of the accoucheur may even enter the orifice of the peritoneal pouch, pressing those parietes before them. If the placenta be still attached, it is felt at the os tincæ or in the cavity of the uterus, and judged to be of unusual size; if partially detached, it allows of great hæmorrhage, which may cease on its removal, if the inverted fundus contract, and be felt firm to the touch; but hæmorrhage continues if the inverted mass be soft, indicating that it still is in a state of inertia. The constitutional symptoms, if not pathognomonic, offer strong indications of what has happened; there is a degree of faintness, great restlessness and distress, or syncope, surpassing what the loss of blood would account for; pains extend to the groins, the loins, and lower part of the abdomen; a fulness is complained of in the pelvis, as if another fœtus remained to be expelled, and corresponding efforts are induced, not solely uterine, but strong expulsive abdominal efforts, such as are readily renewed from their having been so recently exercised in sympathy with the same uterine sensations as the patient continues to experience. Thus the symptoms are found to vary according to the degree of the inversion.[33] After some hours, and in the absence of hæmor-

Fig. 2



[33] "Pro diversa inversionis specie, variantur ipsius signa."—Deleurye, *Theses*, p. 10.

rhage, the urgent symptoms may abate; but retention of urine is sure to happen, if the introversion be in the greatest degree, the fundus descending to the cervix uteri and keeping it dilated; and rectal tenesmus also recurs at intervals, until perchance, under some very violent and instinctive efforts at expulsion, the displacement is carried still further, merging into the divisions to be next explained. It is, however, possible that, if the introversion be overlooked, receive no treatment or no maltreatment, and be not spontaneously righted, it may remain for a continuance,[34] without passing into the next stage of the malady; but if the os uteri be lax and dilated, the malady progresses certainly, and without any powerful efforts.

Perversion[35] is when more or less of the inverted portion of the uterus projects through the os tincæ; in its greatest degree, the whole body of the uterus, as well as the fundus, passes inverted through the os, the cervix only remaining in situ, encircling the contiguous or highest part of the inverted portion, all the rest being uncompressed and unsupported by the uterus, as represented in the marginal Fig. 3. The peritoneal pouch (*a*) is lengthened, and the proper uterine cavity nearly obliterated. The inverted part above the dotted line (*e e*) is surrounded by the cervix, which may constrict it, producing congestion, and even strangulation, in all the rest of the inverted organ situated below the same dotted line. The angle of inflexion[36] (*d d*) is immediately above the cervix in this figure, and is always below the middle of the body of the uterus. The pathological effects which may result from this displacement are obviously so different from those attendant upon the first and second divisions of partial inversion, as to warrant the classification I have adopted, and render it definite, necessary, and of great

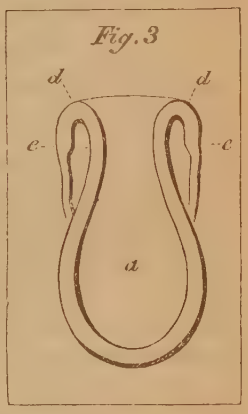


Fig. 3

[34] Refer to Plate 4, and the full description of it.

[35] Synonymes.—L'éversion; chute de la matrice avec renversement.

[36] It is but justice to observe that this expression was first used by Dr. Radford, of Manchester, one of our most able writers upon the subject.

practical utility. In considering the successive steps of the inverting process, we trace the descent of the fundus through the uterine cavity until it projects at the os, fills the vagina, and reaches the external labia; and the process may go on in the same direction to its completion, the inverted fundus and even body of the uterus prolapsing externally, until the encircling cervix descends, under expulsive efforts, to a level with the labia,[37] and becomes apparent under ocular examination. But if the labia resist sufficiently the further descent of the fundus, and part of the body of the uterus remain still uninverted, may not the process be carried to its completion by ascent of the cervix? No author has hinted at this view of the subject, and yet its correctness must be admitted, in order to explain the well-established fact, that where the inverted fundus and body of the uterus are still in the vagina, the cervix is felt high above the pubes,[38] even near to the navel, sometimes taking the situation the fundus would normally occupy, the vagina being proportionately stretched and carried upwards,—changes which can only be explained by supposing that, at a certain stage, the inversion ceased to progress by descent of the fundus, and was continued and completed by ascent of the cervix. If the placenta be still adhering, it precedes the fundus, is felt in the vagina, or observed at the external labia, giving the attendant an impression of its being firmer than usual, and of greater size. In regard to hæmorrhage, the same remarks apply as were noted in

[37] Boivin and Dugès, *Mal. de l'Utérus*, Planche xii., Fig. 1. Dewees (*Midwifery*, p. 471,) remarks, that he has known partial inversion in the third degree (*perversio* with prolapse) pass the labia, and depend externally.

[38] Numerous cases might be quoted to support this opinion, but the following, from Dailliez, (*Thèse, Obs.* 12 *ième*,) will suffice:—"The patient was nearly delivered of her second child, by ten or twelve pains at most, which seemed to be so little profitable, that Baudelocque urged her to delay all expulsive efforts; but the pelvis being very large, the os uteri opened after escape of the waters, and the child's head resting on the perineum, the woman could not forbear, and by one strong effort she expelled the child, and with it the placenta, which presented at the vulva, bringing with it the posterior and superior part of the uterus. In a very short time the inversion was rendered complete, (by which Baudelocque meant that the os and cervix only remained uninverted); the uterus, still retained within the vagina, formed a round and sufficiently firm tumour, at the highest part of which the os uteri could be felt through the abdominal parietes; and the uterus had risen sufficiently above the os pubis to make any one, who had not attentively followed the progress of the displacement, believe that the uterus was in its natural situation." See also foot-note 46.

the preceding division; and if the accoucheur fail to discover, by examination through the abdominal parietes, what has occurred, he may increase the inversion, and even carry it to the greatest degree, in attempting to remove the placenta, which he erroneously considers to be already separated, and remote from the internal fundus of the uterus. If the placenta be already away, a convex tumour occupies the vagina, of a greater or less size, according to the proportion of the body of the organ inverted, having a soft, slightly nodulated surface, bleeding easily under the touch, its highest part encircled by the cervix. If the cervix only remain uninverted, the fundus and body of the uterus may be so large as to fill the bony pelvis, distend the vagina, and render it difficult, if not impracticable, for the accoucheur to reach the cervix; but as often as the inverted mass prolapses at the vulva, the encircling cervix can be felt. The colour of the mass will be florid-red, mulberry, or purplish, varying according to the constriction by the cervix; and at the fundus an uneven surface is distinguishable, answering to the former attachment of the placenta, and from which surface the hæmorrhage has mainly or exclusively proceeded.[39] Examination above the pubes may enable the accoucheur to detect the orifice leading to the peritoneal pouch, formed by the inverted fundus and body of the uterus, or, in the case of external prolapse at the vulva, to convince himself of the absence of the organ from the abdomen.

In this degree of inversion, the constitutional symptoms are much more severe and prominent than in the lesser degrees of partial inversion; the pains extend not only to the groins and the lumbar region, but down the lower limbs, even to the heel, as remarked by Lisfranc;[40] the pulse is small, and often scarcely perceptible; faintness, restlessness, complete syncope, intervene, with distension of the abdomen, great pallor, cold perspiration; the syncope, suppressed pulse, and threatening signs of dissolution, are most certain to happen if the inverted fundus and body of the uterus prolapse externally; retention of urine or dysury is an invariable sequel; and whilst the inverted organ remains still in the vagina, a sense of fulness in the pelvis, with bearing down, and tenesmus, both uterine and rectal, is experienced, and continued

[39] See Section vii. for remarks upon hæmorrhage.

[40] *Leçons de Clinique Chir.*, iii., 387.

until, under greater efforts and the assumption of an erect posture, the whole is prolapsed externally.[41]

Considering the relative position of the internal appendages of the uterus, we find the ligaments and Fallopian tubes drawn further into the peritoneal pouch as the inversion increases, and as it becomes most prolapsed, whilst the ovaria are brought to the margin of the circle formed at the angle of inflexion, next the cervix, approximating each other, but rarely entering the peritoneal pouch.[42]

Although these artificial divisions of partial inversion are required for perspicuity of description, with the view of conveying a knowledge of the subject, and are moreover supported by pathological conditions marking the boundary of each, it should be remembered that they pass imperceptibly the one into the other, from the slighter to the greater degree of displacement. Each of these three divisions exists in various degrees; and to effect the greatest degree of partial inversion, the displacement must progress from the slightest *depression* through all the intermediate stages. More or less time is occupied in this progress; sometimes it is accomplished instantaneously,[43] under violent expulsive efforts, chiefly, if not entirely, abdominal, or, more frequently, under a midwife's management by pulling at the cord. Or the change may be slow from depression to introversion; and this may not be converted into *perversion* for days, or even weeks.[44] It is probable,

[41] See Plate 2, with case and description.

[42] See Plate 3, and description. Saxtorph (*Gesammelte Schriften*, s. 312,) found ovaria and entire Fallopian tubes in the peritoneal pouch in partial inversion, fatal without prolapse.

[43] The most rapid and fatal case is Canolle's, (from *Recueil Périodique de la Soc. de Méd.*, 1798,) so elegantly related by Richerand, (*Nosographie Chir.*, iv., 358,)—"Une jeune fille, devenue enceinte, dérobe à ses parens la connoissance de sa grosesse: une de ses amies la reçoit chez elle à l'époque de ses couches; les douleurs de l'enfantement se font sentir; l'œuf entier se détache à la fois; le fœtus, enveloppé de ses eaux et de ses membranes, est chassé, et tombe avec le placenta; la matrice se renverse, une violente hémorragie se déclare, et la femme meurt en quelques instans entre les bras de son amie. Celle-ci paroît coupable de ce double meurtre; on s'apprêtoit à le venger par le dernier supplice, lorsqu'un chirurgien, défenseur de l'accusée, fit exhumer le cadavre, et, montrant aux juges la matrice renversée, leur prouva que ce renversement étoit la seule cause de la mort."

[44] Desormeaux (*Dict. de Méd.*, xviii., 268,) was first called twenty-one days after delivery, and found the inverted fundus projecting slightly at the os uteri. He conceived that only depression existed at the completion of labour; but the displacement failed

and has been so remarked by numerous authors, that the statements of inversion first occurring at an interval of many days[45] after delivery have been erroneous, and that in such cases the displacement really existed in a slight degree from the time of delivery. It is easier to believe that inversion was present at the termination of labour, and not detected,[46] than that, after the

to be detected, or even suspected, until it progressed to the degree in which he found it. In Teallier's case, (*Med. and Phys. Journal*, li., 259,) where the inverted uterus prolapsed on the twelfth day, under violent efforts to evacuate the bowels, there were strong symptoms indicative of partial inversion for several days after delivery, pain and distension of abdomen, dysury, &c. Voisin, quoted by Velpeau, (*Clin. Chir.*, ii. 420,) was first called to inversion with prolapse, which took place eight days after delivery, no suspicion of such a malady having arisen till his arrival.

[45] "Dans tous ces cas, il n'est pas prouvé que l'accident n' a pas commencé au moment même de la delivrance. On sait, du reste, que le renversement de la matrice peut s'opérer d'une manière lente, graduelle, au point qu'on pourrait, pour ainsi dire, le suivre jour par jour dans ses progrès."—Velpeau, *Clin. Chir.*, ii. 420.

[46] The strongest case published in support of inversion first commencing some days after delivery, is Ané's, (*Recueil Périod. de la Soc. de Méd.* 1797.) The woman was twenty-three years old, and as soon as the child was born there was alarming hæmorrhage, on account of which the placenta was removed by manual aid; and so greatly relaxed was the uterus, that the loss continued severe, and Ané did not leave her till several hours had elapsed, previously satisfying himself that the uterus was well contracted, by feeling it above the pubes. On the third day, the patient was got up and placed on a sofa, when syncope recurred with great loss as before, till the blood appeared on the floor, and the patient seemed dying when the accoucheur arrived: she, however, rallied; and on the twelfth day, when making great efforts to evacuate fæces, which appeared just ready to pass, she perceived the inverted uterus escape at the vulva, equalling in size the head of a new-born infant. Baudelocque effected reduction. Dailliez (*Thèse*, p. 30) remarks very justly upon this case,—that if inversion did not exist in a slight degree at the moment of delivery, we must presume that it commenced, at least, as early as the third day, but did not progress to its final stage till the twelfth; and Baudelocque considered the hæmorrhage on the third day as the result of partial inversion. Depression may exist in a part of the uterus not accessible to the touch exercised through the abdominal parietes; and, moreover, I have known a surgeon, of considerable experience, give assurance of his having felt the contracted uterus above the pubes, and satisfied himself all was right, when there was evidence subsequently that the organ at that very time was totally inverted, the fundus being in the vagina, and the *os* uppermost, rising above the brim of the pelvis. Ané was again attending the same patient in labour, after an interval of six years, and took every precaution to avoid inversion, making no traction by the cord, and limiting himself to the use of friction upon the abdomen to excite uterine action and promote separation of the placenta, which was soon accomplished. For several days the uterus was felt above the pubes, firm and globular, dissipating all fear of inversion. On the seventh day the patient was so well as to walk about her chamber, and the loss of blood had been trivial; but on the thirteenth day, under an effort to

uterus had contracted, and hours or days elapsed, inversion commenced and showed itself quickly in an advanced stage of the displacement. If the uterus be well contracted after complete delivery, it cannot subsequently become inverted, unless again distended by blood, the sudden escape of which, it may be conceived, places the organ in circumstances somewhat similar to those attendant on delivery, and renders inversion just possible. We shall not, however, be far from the exact truth in stating, that inversion can only commence at or soon after labour,[47] and that most of the recorded instances of its occurring after the lapse of days or weeks, may be placed to the account of oversight, or error in diagnosis.

Total inversion is where the cervix, as well as all the rest of the uterus, is inverted, as in Fig. 4.[48] The possibility of this occurrence has been denied by a majority of the earlier writers upon the subject, particularly by Baudelocque[49] and his followers: but

evacuate the bowels, the uterus became inverted, and prolapsed externally, reaching half way towards the knees, and equalling the size of a fœtal head at full term. Baudelocque arrived in two or three hours, and succeeded in reducing the inversion; recovery ensued. It is said that Ané, at each visit for the first six days after delivery, examined above the pubes, and recognised the uterus of a globular form, without the slightest depression. Still Dailliez observes,—"Nous pensons d'ailleurs que ces renversemens complets qui se manifestent aussi tard, ont existé long-temps avant d'une manière incomplète, et qu'ils ont presque toujours commencés ou dans le moment de la délivrance, ou immediatement après."—*Thèse*, p. 32.

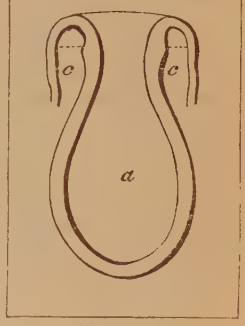
[47] "Nunquam nisi in aut mox post partum accidere potest."—*Ruyschii Obs. Anat. Chir.*, p. 16.

[48] In Fig. 4, all below the transverse dotted line is the uterus totally inverted; and above it, a small portion of the vagina is situated, terminating as a cul-de-sac. In parturition, when the cervix uteri is perfectly relaxed and dilated, all trace of the projecting *os tincæ* is obliterated, and the vagina and cervix uteri form one continued cavity, without any distinct boundary, until the cervix again contracts.

[49] "Tant qu'il y a une portion de la matrice susceptible de se retourner et qui ne s'est pas retournée, le renversement se nomme incomplet; il n'est complet que quand tout ce qui peut se retourner est retourné..... Une seule portion de la matrice ne peut se renverser ou se retourner; c'est celle qui est au dessous de l'insertion du vagin au museau de tanche, consequemment le museau de tanche même, qui forme après le renversement ce bourrelet souvent peu saillant, qui entoure en manière de bague ou d'anneau le pédicule de la tumeur formée par la matrice renversée."—Dailliez, *Thèse*, p. 27. "Confirmat morbum exploratio vaginalis, quâ palpatur tumor è vagina prodiens cinctusque orificio uteri numquam inverso."—Lassabe, *Thesis*, p. 8. Moreau (*Des Accouchemens*, i., 247) and Ferrand-Demissols, (*Thèse*, p. 16,) hold the same doctrine.

its occurrence happily admits of demonstration, and a long list of the highest authorities can be quoted in support of it:—Levret,[50] Sabatier,[51] Lefaucheux,[52] Leroux,[53] Boivin and Dugès,[54] Dewees,[55] Desormeaux,[56] Radford,[57] and Vidal-de-Cassis;[58] so the question ought to be set for ever at rest. The entire uterus was indubitably inverted, including the cervix, in my published case;[59] the termination of the vaginal membrane in the uterine was very distinguishable, not only to the touch, but under ocular inspection, and was without any overlapping, without any uninverted edge,—a smooth circular thickening[60] marking the situation of the inverted cervix, and the boundary of the vagina. Although totally inverted, the uterus may still be retained within the labia: my own experience on this point is confirmed by the clearly-narrated case by Horteloup.[61]

Fig. 4



[50] "Renversement total de son fond et de son corps, même de son col par son orifice propre."—*Cure des Polypes*, section iv.

[51] "Tout ce viscère se retourne sur lui-même, passe par son orifice, entraine une partié du vagin avec lui, et descend jusqu' entre les cuisses de la malade."—*Mem. de l'Acad. Chir.*, 8vo. edition, iii., 359.

[52] *Diss. sur les Tumeurs*, &c., p. 47.

[53] *Obs. sur les Pertes*, p. 165.

[54] "Le col lui-même a été entraîné dans cette évolution à laquelle le museau de tanche seul a échappé."—*Mal. de l'Utérus*, i., 221.

[55] "By a complete inversion, I mean the passing of the fundus and body of the uterus through the *os*, or being turned entirely inside out to the very mouth of this organ."—*System of Midwifery*, p. 470.

[56] "Dans quelques cas, la partie superieure du vagin elle-même est renversée; l'orifice forme alors sur le pédicule une saillie circulaire au-dessus de laquelle on sent la portion vaginale plus molle que le reste de la tumeur."—*Dict. de Méd.*, xviii., 272.

[57] "In complete inversion, the cervix is not inflected upon itself, or embraced by the *os uteri*, but this orifice is effaced, and the vagina is inverted to a greater or less extent."—*Essay on Inversio Uteri*, p. 1.

[58] *Pathologie Externe*, v., 781.

[59] *Provincial Medical and Surgical Journal*, June 12, 1844, p. 154.

[60] See Plate 1 with description; the letter (*a*) marks the thickening answering to the cervix and os uteri.

[61] "On voyait les ligamens ronds, les ligamens larges entrainés dans l'entonnoir que formait la matrice; les ovaires étaient placés symétriquement sur le rebord circulaire posterieur du renversement; les trompes flottaient libres dans le bassin,

The patient, thirty-two years of age, was brought into the Hotel Dieu at Paris, in a dying condition, three hours after delivery; she soon expired, the hæmorrhage having ceased only when the system was deprived of all its blood. On autopsy the uterus, totally inverted, was in the vagina, the inverted cervix being continuous with it, and presenting no margin to indicate its existence; the circular aperture at the entrance of the peritoneal pouch, as represented in the plates given in the *Repertoire,* mounted considerably above the pubes towards the umbilicus, proving that the cervix and part of the body of the uterus must have ascended in the progress of the inversion to its completion, as before mentioned in this section in speaking of partial inversion in the greatest degree. Horteloup observes that, had the cause of the hæmorrhage been known, the patient might have been saved; but the inversion was first detected *post mortem*.

Total inversion of the womb can only happen by the displacement passing through all the minor degrees, beginning with the slightest depression. It may happen instantaneously, when the os tincæ is greatly dilated, the whole of the uterus relaxed or in a state of inertia, and the expulsive abdominal efforts (*nisus depressorius*) are powerfully and, as respects the patient, irresistibly exerted, driving down and expelling the organ as it is inverted, until it depends many inches at the labia, or reaches nearly to the knees of the patient,[62] who exclaims that "her very bowels are

et la masse intestinale recouvrait le tout. . . . . En introduisant le doigt dans le vagin, on rencontrait le sommet de la matrice, dont la surface mamelonnée indiquait l'insertion du placenta; puis glissant autour de ce cône renversé, on arrivait à un cul-de-sac circulaire que formait le vagin à son insertion sur le col; celui-ci effacé ne présentait pas même un rebord qui pût indiquer son existence."—*Rep. d'Anat. et de Phys. Pathologique*, ii., 229. A wax model of the inverted uterus is preserved in the Cabinet de la Faculté de Medécine de Paris; and Plate 9 of the *Repertoire* gives two illustrative views, one of the abdominal aspect, and the other of a section in the mesian line. Dewees observes, "I have known the inversion to be complete, and the fundus not escape from the vagina."—*System of Midwifery*, p. 471.

[62] In all these extreme cases of prolapse of the inverted organ, the vagina is inverted more or less, and drawn down with the uterus, forming the neck of the pyriform tumour. The bladder, though rarely altered in its situation, may, if the vagina be extensively inverted, be drawn downwards and backwards, so as to become involved with the highest part of the tumour; and even the urethra may be so changed in its direction, that the catheter passes from its *ostium* downwards to reach the bladder. Mr. Mandby, an intelligent surgeon of long experience, has communicated to me a case of complete inversion with prolapse, in which, before attempting

passing from her;" or it may be effected by gradual steps, when the rationale of its progress is different, as I shall endeavour to explain in considering fully the efficient causes.

One great source of error and obscurity has been the confounding of prolapse with inversion. Most authors have defined complete or total inversion to be external prolapse of the organ: prolapse, as I have already affirmed, may be added to the third degree of partial inversion; it is a still more frequent complication of total inversion; but in either of these extreme degrees of displacement, prolapse may not occur speedily after delivery, but after an interval of several hours, days, or even a week or two.[63] It brings less immediate danger, as its occurrence is more remote from the delivery; but in all instances, external prolapse aggravates the symptoms and increases the danger, the patient often dying in an hour or two, from hæmorrhage, pain, syncope, and convulsions. The prolapse remaining external, the uterus loses its natural warmth if not excluded from the atmosphere; and on examining as deep as the

Fig. 5

reduction, he thought it necessary to use the catheter; and in searching for the entrance to the meatus urinarius, he found it had been drawn down so as to be quite visible below the superior commissure of the external labia, and the catheter took a direction contrary to what is usual to reach the bladder.

[63] Fig. 5 is a plan of external prolapse, with total inversion of the uterus and inversion of part of the vagina:—(*a*) os pubis; (*b*) os sacrum; (*c*) bladder; (*d*) rectum; (*e*) peritoneal pouch of the inverted uterus; (*f*) the pouch continued by the inverted vagina; the boundary of the uterus is marked by a thickening, being the only remaining

uninverted portion of the vagina will permit, the accoucheur may feel the round ligaments,[64] in each lateral aspect, as very tense cords, and trace them within towards the brim of the pelvis. Not only the round and broad ligaments, but even the ovaries and entire Fallopian tubes, may occupy the peritoneal pouch; if called soon after delivery, we not unfrequently find the placenta still adhering to the inverted fundus, and lying between the lower extremities of the almost lifeless patient.[65]

trace of the os and cervix. The external prolapse, if not immediate, most often takes place within a few hours: cases are on record of one day elapsing, (Troussel, Church); of two days, (Delmas, Elkington); of four days, under my own observation; of eight, (A. Hunter, Voisin); of ten, (Teallier, Le Blanc); of twelve and thirteen, (Ané); of fourteen, (Cleghorn.)

[64] In the case I have detailed, on introducing the finger into the vagina as far as was admissible, I could feel each round ligament, tense and stretched, like a firm cord; and on pulling down the prolapsed organ a little lower, I produced pain at the groins. The only writer I have met with who mentions having felt the round ligaments, is Von Herder. The inverted prolapse was partial in the third degree, and the midwife had cut into it with the scissors, causing much hæmorrhage before his arrival:—"Ich fühlte wie dies organ durch *die ligamenta rotunda, welche angespannt werden*, innig verwachsen war."—*Diagnost. praktische Beiträge*, p. 124.

[65] Viardel (*Prat. des Accouchemens*, p. 200,) published a Plate representing the inverted uterus prolapsed, with the placenta still adhering to its fundus; the pyriform shape is well shown, and coincides precisely with Plate 1 of this Essay, representing the shape and size of the organ at one month after delivery. The first representation of the prolapsed and inverted uterus was given by Ruysch, (*Obs. Anat. Chir.*, Fig. 12, p. 17.) The prolapse is nearly seven inches long, and six in transverse diameter, and being in a relaxed state, the open orifices of the uterine veins are observable. The reasons for the representation are assigned by the author:—"Multoties *Obstetrices* non solum, verum etiam *Chirurgos, imò et Medicos* hac in re esse hallucinatos, non est, lector benevole, cur dubites. Nuper adhuc me arcessi jussit Chirurgus quidam, qui in eodem casu conjicere haud potuerat, qualis esset tumor è corpore propendens. Aliquoties me quoque vocarunt Obstetrices, mirabundæ, quod viderent ejusmodi tumores à partu sese exserentes. Nonnullæ mihi venienti dixere, hic videbis molam magnam, utero firmiter adhærentem: aliæ, hic habemus partum fœtus monstrosi, &c. Hunc ergò affectum delineavi, ut unicuique Chirurgo, qui delineationem viderit, magis in memoria hæreret, et ne quis amplius in re tanti momenti, et subitam restitutionem exposcente affectu, deciperetur." Ruysch's hopes were not visionary, for Morgagni relates that a surgeon came to seek his opinion on a novel case, to whom he showed Ruysch's delineation, and advised him to hasten back and replace the inverted uterus, which was done within an hour from its occurrence, and the lady recovered. (*Grieve's Celsus*, note in Preface, p. 12.) The plans of inversio by Levret, Fries, and Dewees, deserve to be named amongst the useful graphic illustrations of this subject. Fries also gives a miniature Plate of the inverted prolapse, which is copied in *Froriep's Chirurg. Kupfertafeln*, Taf. 42, Fig. 2. The Plate of Boivin and Dugès has already been referred to, showing external prolapse of partial

It will be observed that in all I have stated regarding the different stages of uterine inversion, it commences from the superior part of the organ, usually the fundus, and progresses through the cervix and os to its completion; there is no evidence of the inversion progressing in the opposite direction, that is, from the cervix or the contiguous part of the body of the uterus, towards the fundus, although, with a very dilated cervix, it would appear very possible, in a mechanical view, to find the progress of the displacement to be from the cervix towards the fundus.[66] When lateral depression is the first step, the fundus follows, before the inversion is completed; and, at all events, the cervix, and the body of the uterine organ continuous with it, are the last parts that become inverted. Several of the domesticated quadrupeds[67] are liable to uterine inversion, with prolapse; and I have reason to believe that the progress of the malady in them is often, if not uniformly, from the vaginal termination of the uterus towards the fundus, the reverse of what happens with woman. Many authors who have attempted to describe the progress of inversion, have

inversion. Those of Heister, (*Inst. Chir.* ii., Plate 36, Fig. 3,) Levret, (*Des Polypes*, Plate 3, Fig. 4,) Burton, (*System of Midwifery*,) and Jörg, (*Lehrb. für Hebammen*, Taf. 9, Fig. 2,) are quite unworthy of the subject, and on being referred to will furnish the best apology for fresh illustrations. Dr. William Hunter has given a Plate of the gravid uterus at full term, turned inside out, to show its anatomical structure; and this has been often referred to as an inverted uterus *post partum:* if it were so, the position of the Plate should have been reversed, with the fundus lowest. All that the author states is the following:—"Mulier hæc, post partum neutiquam difficilem, in animi deliquium inciderat, causa quidem adstantes latente; horis autem duabus a partu nondum lapsis, illam mors abstulit." (*Anat. Uteri Gravidi*, Tab. xv., Fig. 1.)

[66] H. Von Sanden described the progress correctly long ago:—"Inversus non quoad orificium sed quoad fundum, uterus descenderet extra vaginam, propendens tumore livescente, cucurbitam duarum librarum æquans."—*Obs. de Prolaps. Uteri Inversi*, p. 25.

[67] The mare, cow, and sheep, not very unfrequently suffer uterine inversion with prolapse. Le Blanc, who commenced in the veterinary line, but afterwards became *the great surgeon and lithotomist*, states that he had seen two hundred instances of inverted uterus in the cow. (Ferrand-Demissols, *Thèse*, p. 40.) A good representation of the totally inverted organ, prolapsing two or three feet, is given by Skellet. (*Treatise*, &c., p. 176.) After reduction, the same treatment is still adopted which Ruysch described:—"Bidenti scilicet furcâ pertundentes cutem, &c." (*Advers. Anat.*, Dec. 2, p. 38.) A veterinary surgeon lately, within my knowledge, met with uterine inversion in a mare, complicated with rupture of the organ near the vagina, and prolapse of several feet of intestine: he reduced the intestine, and subsequently the inverted uterus, and recovery speedily followed.

compared it to "the turning of the finger of a glove inside out" on removing it from the hand; the one is just the reverse of the other, in the human frame, as to the successive changes, although the effect may be the same when, in both instances, the inversion is rendered complete. The comparison would be correct only on the condition that uterine inversion progressed from the cervix to the fundus.

In all degrees of inversion, there is a concavity or pouch lined with peritoneum, and open towards the general peritoneal cavity. In simple depression, the intestines rest in the concavity; and as the pouch or cul-de-sac increases, the intestines may, if the opening into it be large, occupy this pouch, so that in total inversion, with prolapse, they may actually descend beyond the external labia, still resting in the peritoneal bag which the inversion has occasioned. It is easy to conceive that, under these circumstances, a portion of intestine may become incarcerated,[68]

[68] Van der Wiel has been quoted for having observed the intestines occupying the peritoneal pouch of the inverted uterus, and their incarceration in this situation; but it does not appear to me that he has been correctly quoted. Called to a robust woman, who had died the day before, half an hour after delivery, he found the inverted uterus prolapsed between the lower limbs:—"Nos autem inversum hoc corpus bene perspectum, facto in fundo foramine, *in suum reduximus locum*, atque tum illico per dictum foramen ipsa sensimus intestina." After having made an opening in the fundus, and then reduced the uterus, what could he feel through the opening but the intestines?—which could not occupy the peritoneal pouch after the inversion had been replaced. (*Obs. Rariores*, i., 292.) The intestines have, however, been repeatedly found in the peritoneal cul-de-sac formed by the inverted uterus. Baudelocque used to exhibit at his lectures a drawing illustrative of this fact; and Murat, his pupil, refers to the same, by stating—"J'ai souvent vu dans son cabinet le dessin d'une matrice renversée incomplètement, dont la cavité contenait plusieurs anses d'intestins." (*Dict. des Sc. Med.*, xlvii., 478–484.) "Herniæ incarceratæ omnia symptomata parere potest in uteri cavo incarceratus intestini gyrus." (Lassabe, *Thesis*, p. 9.) Meissner attributes to this cause the syncope and dangerous convulsions. (*Disloc. der Gebärm.* iii., 14.) In Dalmas' case, a fold of intestine occupied the new cavity formed by the inverted fundus uteri. (Ferrand-Demissols, *Thèse;* p. 33.) The term *enterocele hysterica* has been applied to this malady. Dailliez, referring to the early complications of uterine inversion, observes:—"Une anse d'intestin peut suivre le fond de cet organe, s'insinuer dans la cavité dont l'entrée est d'abord très-large, s'y étrangler, comme on l'a observé à la suite de la rupture de la matrice, et donner lieu à des nouveaux accidens qu'on n'a régardés jusqu'ici que comme sympathiques. Les douleurs d'entrailles, la tuméfaction du ventre, les nausées, les vomissemens, le hoquet, attribués tant de fois au renversement de la matrice, auraient bien pu ne dépendre chez quelques femmes que de cet étranglement." (*Thèse*, p. 80.)

and present all the circumstances, and induce all the symptoms and dangers, of strangulated hernia. These effects are described in general terms by many writers, but I have not found any particular case in which intestinal strangulation happened, as developed by undoubted symptoms, nor proof of death from this cause, amongst the numerous examples of autopsy which will be noticed in the course of this essay.

The symptoms accompanying each degree of inversion will be further reviewed in considering the pathological effects. In recent inversion, the lining membrane of the uterus, converted to its external covering, possesses little sensibility, unless congested, inflamed, or otherwise morbidly affected, in addition to the mere displacement. In a case of inverted uterus, externally prolapsed for several days, I experimented[69] by touching the membrane, pressing it with my finger, pinching it and pressing my finger nail into it, even pricking it with a pin, but no sensation was produced, nor any inconvenience experienced by the patient; it being wintry weather, I placed snow on the exposed lining membrane of the uterus, but it gave rise to no sensation of cold; at the part, however, answering to the os uteri, and at the contiguous part of the vaginal membrane, cold was felt by the patient from the contact of the snow. It should be recollected that this was a case of total inversion, and that there was no cervix producing constriction upon the uppermost part of the uterus. The muscular coat of the inverted uterus may be made to contract by change of temperature, by pressure, or other source of irritation, applied to the mucous membrane covering it. Lisfranc[70] observes that the uterus is sensible after a manner peculiar to itself, and according to the different agents acting upon it, but he does not explain what this peculiar manner is. These remarks apply only to the recently inverted organ.

If the uterine inversion be not corrected, and the patient survive the first shock to the system, the hæmorrhage ceases, and rarely recurs until the return of menstruation; the nervous symptoms also abate; the discharge, which for a time is always fetid and offensive, diminishes, subsiding to a common state of leucorrhœa; and the uterus speedily diminishes in volume, resting in the vagina,

[69] *Provincial Medical and Surgical Journal*, June 12, 1844, p. 155.

[70] "La matrice sent d'une manière différente, suivant les agents qui sont appliqués sur elle." (*Clin. Chir.*, iii., 205.)

and obeying the same law as to its decrement,[71] as if it were not inverted. During the period of the patient's giving suck to her infant, should she be able to do so, the health is little affected. In two or three weeks the uterus will be reduced to one half the size it presented shortly after delivery; and the decreasing progress goes on so regularly and uniformly, that after a lapse of five or six months, if no morbid conditions consequent on the displacement arise to interfere with and retard it, the uterus will have subsided to its normal unimpregnated size, and sometimes has been found even smaller than before impregnation.

Throughout this section I have had in view the occurrence of uterine inversion after delivery at the full period of utero-gestation; it is less liable to arise or be produced in a premature confinement: still Meissner[72] relates a case where the inversion occurred after delivery in the eighth month; and although he denies that inversion can follow a miscarriage at four months, we have the authority of Lisfranc[73] for its taking place at even an earlier date, and may hence conclude that it is possible, however infrequent, for the displacement to be consequent on the expulsion of the ovum or fœtus at any period of utero-gestation after the first three months.

SECTION III.

ON CHRONIC INVERSION OF THE UTERUS AFTER PARTURITION.

When the inversion remains unreduced, and sufficient time has elapsed for the organ to diminish till it approaches its natural unimpregnated size, it may be regarded as chronic. The com-

[71] Having gradually increased in size during pregnancy, the uterus soon after delivery commences a contrary process, and daily diminishes until it has regained about its normal unimpregnated size. This is what I mean by the decrement of the uterus, a change which has not received sufficient attention; and I am consequently unable to state with precision how long the decreasing process goes on. Dr. Rigby, of London, observes to me, that during the first thirty-six or forty-eight hours after delivery, the uterus slowly increases in size, and at the end of this period it begins to diminish, and continues to do so for some weeks; but remains softer and larger than in the virgin state, and does not attain its original size and hardness until the menstrual part of a woman's life has terminated. (*System of Midwifery*, p. 22.)

[72] *Disloc. der Gebärm., Theil* iii., p. 26.

[73] *Clinique Chir.* iii., 383.

mencement of the chronic stage has reference, therefore, both to the duration of the malady and the size of the organ, but in neither of these respects can perfect precision be attained; and we must agree to regard the disease as chronic, when five or six months have elapsed.

By far the most frequent form of chronic inversion is the greatest degree of the partial displacement, (*perversio,*) in which the cervix only remains uninverted; in a practical view, it may be considered that the remarks in this section are applicable solely to this division of the disease, unless otherwise specified. There is no evidence of depression persisting till the chronic stage; in the rare instances of *introversion* doing so, of which Plate 4[74] is a doubtful example, detection would be scarcely practicable, unless the os uteri were sufficiently open to allow of a digital examination, and the absence of the fundus from its proper site were ascertainable by the means hereafter to be pointed out under diagnosis;[75] for the

[74] See the description of Plate 4, which refers to a preparation preserved in the Hunterian Museum at Glasgow, and of which the late Dr. Allan Burns made me the drawing just before his decease.

[75] In chronic inversion, when the uterus is become of small size, and the inversion itself has caused the fundus to be lower than natural, we can rarely expect to feel the organ through the abdominal parietes, and thus ascertain the inversion; but the following case, related by Baudelocque, and without doubt faithfully related, proves that sometimes we may gain satisfactory evidence in this way, even after several years' continuance of the malady, the emaciated state of the patient facilitating the investigation:—The inversion occurred after a first delivery, at about twenty-one years of age; and after suffering losses for eight or nine years, the patient sought Baudelocque's advice on that account. The patient was very thin, almost in a state of marasmus, profuse mucous discharge continuing in the intervals between the coloured losses. The skin was flaccid, the countenance pale and sallow, and the assemblage of symptoms announced a fatal result to be near. Some surgeons had attributed the discharges, white and red, to a polypus; others, so situated that they had not the courage to avow it, could only ascribe the symptoms to an inversion of the womb, which they had failed to recognize during their several visits within the first forty days after delivery, on account of copious flooding. (*Pertes de sang très inquietantes.*) Baudelocque found a pyriform tumour in the vagina, which he estimated to be about an inch and three quarters in length, and an inch and a quarter in thickness; it was rather flattened on its anterior and posterior aspects, and its surface was smooth, polished, covered with a sanguineous mucus, and slightly painful under the touch; its pedicle was encircled by a soft band, within which the finger could be admitted to the depth of the third of an inch. The hypogastric region was so yielding, that on pressing deeply towards the pelvis, Baudelocque could, without difficulty, discover through the abdominal parietes all the characteristic signs of an inversion of the womb, of long standing. (Dailliez, *Thèse, Obs.* 24, p. 73.)

leucorrhæa, frequent menorrhagia, irritation, and uterine tenesmus, would not prove characteristic of the disease.

If the patient pass safely over the first few weeks of the inversion, she often suffers only slightly, and rarely or never dangerously, until the chronic stage has arrived, and the menstrual loss returned; and when the disease has been entirely overlooked at the commencement, it is usually at the expiration of six or eight months after delivery, that the symptoms become so severe, so distressing, and sometimes so perilous, as to compel the patient to seek medical advice.

The tumour occupying the vagina is of a pale-red or florid colour,[76] when observable by aid of the speculum; its surface is smooth, covered with an abundant mucous secretion, somewhat sensible to pressure or mechanical violence,[77] and bleeds easily from the slightest injury. If brought under view during the menstrual period, the coloured fluid is seen to ooze from every part of the surface, and an abundant leucorrheal discharge is furnished during all the intervening period. The tumour is of great density and firmness, as distinguished by the finger through the enveloping membrane; but it is larger and softer in its texture during menstruation; the size and shape vary considerably, but less so than has been conceived by many writers, as I can readily demonstrate. In the patient whose case is represented in Plate 7,[78] and who died nine months after delivery, exhausted by discharges, only a conical portion of the inverted fundus projected at the os uteri,

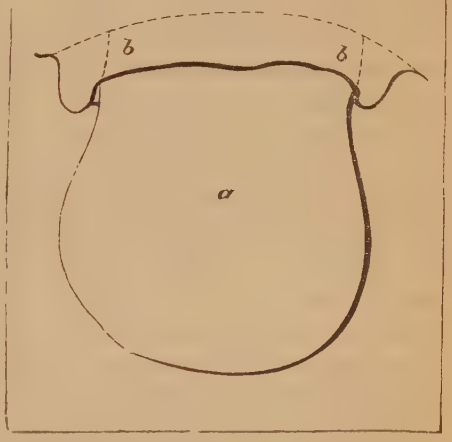
[76] Dr. Ingleby compared the colour and appearance of the tumour, in a case I examined with him, to a ripe red cherry.

[77] Some writers have stated that they found the uterine tumour in chronic inversion insensible, but a majority describe it to be sensible under pressure; and in a state of congestion, chronic irritation, or inflammation, it is very sensible, and very painful on pressure. In a case of partial inversion of five years' standing, and not detected till after this lapse of time, (the patient twenty-four years of age, and the tumour still in the vagina,) its surface was quite sensible on being scratched with the nail, by Klüge, of Berlin; and subsequently the uterine surface was repeatedly punctured slightly with a knitting-needle, and the patient was cognizant of each impression. (*Mit einer Strick-nadel punctirte ich sanft die Geschwulst mehrere mal, immer gab die Kranke die jedesmalige Anzahl der Puncte an.*) (Hauck, in *Caspar's Wochenschrift*, iii., 703.) When prolapsed for many years, the inverted uterus may be very sensible to cold from access of the external air, as stated by La Motte. (*Traité des Accouchemens*, p. 653.)

[78] See full description of Plate 7, with the case. The original preparation is in the Anatomical Museum of Queen's College, Birmingham.

which appears dilated, and the rest of the inversion is contained above the cervix uteri. But in all instances where the cervix only remains uninverted, the tumour is smaller at its highest part next the cervix, and increases in size as you approach its centre: whatever some writers have stated to the contrary,[79] I find no exception to the correctness of the remark, that the circumference of the tumour is greatest midway between its two extremities, and that a gradual diminution of size is observable as you trace the tumour upwards towards the encircling cervix. In a case of chronic inversion which I lately examined, about nine months after delivery, and where the patient was exhausted to the utmost by hæmorrhage, I judged the tumour occupying the vagina to be of the size (*a*) of Fig. 6, quite sensible to the touch, and even tender from repeated pressure under recent examinations and ineffectual attempts at reduction; the encircling cervix (*b b*) was sufficiently yielding to allow my finger to pass within it and to reach the angle of inflexion of this membrane, which I estimated at from one-third to half an inch.[80] An instance of fatal result is afforded in Plate 5,[81] showing

Fig. 6

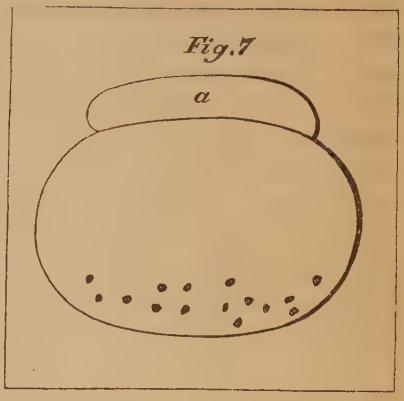


[79] Newnham remarks that the inverted uterus is broader above than below. (*Essay*, p. 53.) Samuel Cooper promulgates the same remark. (*Dict. of Surgery*, seventh edition, p. 1380.) Herbiniaux has been quoted to the same effect; but what he does remark is quite correct, that the partially inverted uterus is very large above, where it is surrounded by the cervix, whilst polypus is larger at its lowest part than above. (*Traité des Accouchemens*, ii., 58.) All these remarks are valueless, unless it be specified whether they apply to recent or chronic inversion, a distinction not made by any of these several authors.

[80] The inverted portion of the uterus in this case has since been removed by ligature below the cervix with perfect success, by Dr. Ingleby.

[81] See full description of Plate 5, which is after an original drawing recently made

the size of the tumour at the end of two years from the occurrence of the inversion; but some allowance must be made in this case for the diminution of size occasioned by the action of spirit, the specimen now in the Musée Dupuytren, at Paris, having been preserved above forty years.[82] Further information may be derived from Fig. 7, referring to the shape and size of the inverted uterus after the lapse of five years, as found by Boivin and Dugès[83] on examination of the patient, who was possessed of tolerable health and great corpulency, but complained of the absence of menstruation, experiencing no other vaginal loss than a reddish mucus for a few hours every fifteen or twenty days. The tumour occupied the centre of the vagina, and was estimated to be above an inch and a quarter in its transverse diameter, flattened and not painful on pressure; exposed by the aid of a speculum, the surface of the tumour was observed to be of a pale-red colour, marked by numerous small spots of ecchymosis, resembling flea-bites. The os tincæ (*a*) firmly embraced the uppermost part of the tumour; dragging pains were still experienced in the loins and in the groins, and there was a frequent call to micturate when erect, but not during recumbency:

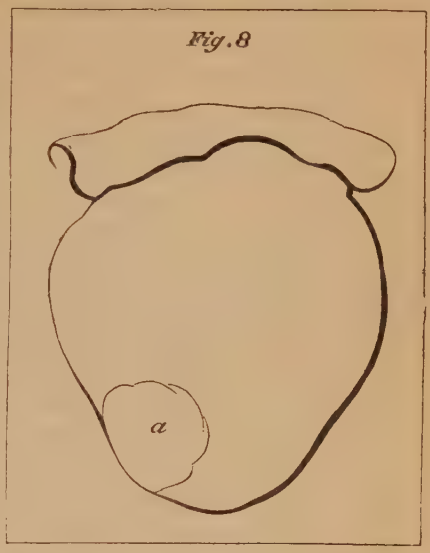


by M. Biot, of Paris, from the preparation. A representation of the same specimen was published by Ségard in 1804. (*Diss. sur les Polypes Utérins*, Plate 2, p. 31.) The only other published plates of chronic inversion that I am able to refer to are by Baillie, (*Engravings of Morbid Anatomy*, Fasc. ix., Plate 5, Fig 2,) and Denman, (*Introduction to Midwifery*, 4to., Plate 12): these both refer to the same specimen, which is now in the Museum at the London Hospital. Baillie's Plate is copied in *Froriep's Chir. Tafeln*, Taf. 42, Fig. 1. A reference to these plates will confirm the account I shall give of the shape and size of the tumour in chronic inversion.

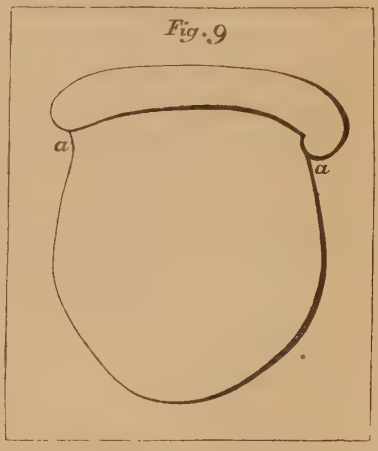
[82] As the spirit acts on all the surface, the shape of the part remains the same, notwithstanding the diminution of size. The same explanation is applicable to Dr. Mackenzie's two specimens, in which the inverted uteri are smaller than during the life of the patients. Plate 5 of this Essay is smaller than the representation given by Ségard; but in all other respects the two, being original drawings, are almost identical.

[83] *Mal. de l'Utérus;* Atlas, Plate 12, Fig. 2.

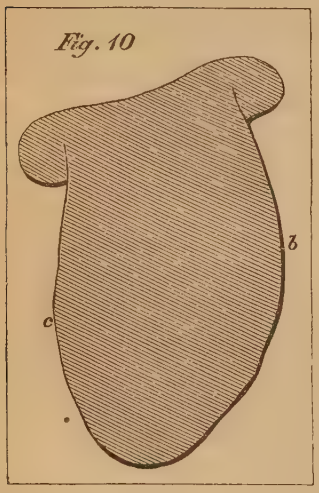
besides these symptoms, the patient made no other complaint, except of her obesity.

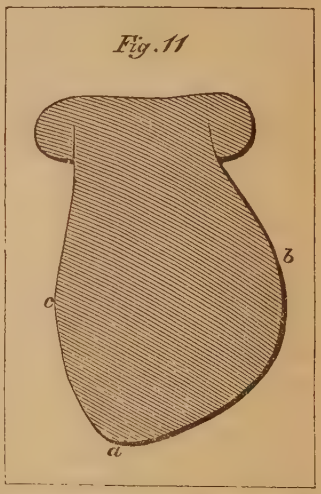


I am enabled to exhibit, with still more precision, the size and shape of the chronically inverted uterus, through the liberality of Dr. Mackenzie, of Glasgow, who has long preserved in his pathological collection two specimens of this disease, which he met with in his dissecting rooms between the years 1819 and 1824. The bodies appeared to be those of women pretty far advanced in life, about fifty years of age, but no information could be gained as to their previous history. Fig. 8 is a correct outline of one of these uteri, a front view, measuring an inch and three-quarters in transverse diameter, two inches in length, four and a quarter in circumference next the cervix, and five in its greatest circumference, which occurs midway between the upper and lower limits of the tumour occupying the vagina. The lower half is conical, with the apex downwards, and the upper half diminishes gradually towards the cervix. There is an indentation at (*a*), which probably resulted from pressure in attempts at reduction. The other of Dr. Mackenzie's specimens is smaller, answering in the anterior view to Fig. 9, measuring an inch and a half in breadth and length, and just above



four inches in its greatest circumference, from which it diminishes gradually towards the cervix, where at (*a a*) the circumference is three inches and a quarter. I should not give such a dry detail of dimensions in these cases, were it not for the value I conceive to be attached to them in respect to diagnosis. The coincidence in shape in these two specimens, in regard to the downward apex, the midway situation of the largest circumference, and the gradual narrowing thence towards the cervix, indicate that this is the shape the inverted organ naturally takes, when, unreduced, it subsides into the chronic stage, without undergoing any serious morbid change in its texture from inflammation, congestion, or pressure. There can be little doubt that both the women to whom these specimens separately refer, had passed the menstruating age; the uterine membrane forming the covering of each tumour is smooth, and scarcely different from that lining the vagina, and the firm texture of each tumour corresponds to the presumed period of life. But the shape of the uterus in these examples of chronic inversion would be inadequately conveyed to the reader without a lateral view being given, obtained by a medio-longitudinal section, which I have accomplished by making casts of each in plaster of Paris, and then dividing these casts. Figs. 10 and 11 consequently





give an exact view of the cut surface presented by the left half of each cast of Figs. 8 and 9; the curved line to the right (*b b*)

representing the convexity of the anterior surface of each tumour, and the line (*c c*) the flat posterior surface. In Fig. 11 the apex of the inverted fundus comes nearly to a pointed shape (*a*.) The width from (*b*) to (*c*) is little more than one inch in either figure, showing how much less the antero-posterior diameter of each uterus is than the transverse, and proving the inverted organ in these cases to be flattened and not circular.

When, as in several of the cases just referred to, the symptoms induced by the displaced organ are mild and such as prove compatible with the continuance of active life, there is an uniformity of shape and size in the chronic stage; but where the symptoms prove severe, and profuse losses, sanguineous or mucous, wear down the strength of the patient, and chronic irritation or inflammation of the uterine tissue itself is kept up, or ulceration, indicated by purulent discharge, and determined by the constriction of the encircling cervix, occurs, it cannot be expected that the uterine tumour, still remaining in the vagina, should present just the shape and size I have represented. We accordingly find many examples of the chronically inverted uterus, where from congestion, irritation, and inflammation, the organ is of much larger size, or may never have subsided, so as to approximate to the natural size of the unimpregnated organ. In Dr. Symonds' case,[84] a female only eighteen years of age, and the inversion partial and of two and a half years' duration, the tumour in the vagina was two and a half inches long, although an inch and a quarter only in transverse diameter; it was broader at its inferior extremity, and gradually tapering towards the cervix. In Esselman's case,[85] of twelve years' standing, the uterus was found in the vagina the size of a large pear. Hauck,[86] at five years, compares the inversion to a German pippin. The size of the uterine tumour has also been compared to that of a small melon, or to the fœtal head of six or seven months; but in the instances of the vaginal tumour being of very large size, we may conjecture that often other diseases have been mistaken for chronic uterine inversion, which never presents a tumour of enormously large size in the vagina, such as authors have frequently described under this denomination.

[84] *London Medical Gazette*, viii., 242.
[85] *Ibid*, xxxiv., 510.
[86] *Caspar's Wochenschrift*, iii., 702.

In the instances, so very rare, of *total inversion* becoming chronic, the symptoms are milder, and the tumour is usually pyriform,[87] hanging dependant from the upper part of the vagina, like the tongue of a bell, the vagina being perfectly closed superiorly, and forming a circular cul-de-sac around the neck of the uterine tumour. In the singular example represented in Plate 6,[88] the tumour is nearly globular, or approaching the ordinary shape of an apple; a variation deserving of particular notice with a view to correct diagnosis.

In chronic inversion, whether partial or total, the uterus is subject to descent and prolapse, followed by all the complications, inconveniences, and dangers, attendant on similar displacements of the uninverted uterus; indeed, when the patient lives long, and particularly in the lower class of society, prolapse of the inverted organ takes place sooner or later in a great proportion of instances. In Placido Portal's patient, who had been thrice received into the hospital, on account of uterine disease, without the nature of the malady being correctly ascertained, the uterus prolapsed externally at the expiration of three years, under an effort at defecation; it was of a pyriform shape, with the base depending, and equalling in size a very small melon.[89] The case most minutely and ably described by Mr. Boyer Harrison,[90] and subsequently operated upon with success, was of five years' standing; the patient was pale and ex-sanguineous, and becoming hectic, from vaginal discharges; on forcing down whilst in the sitting posture, and at every visit to the *commode,* she protruded the inverted uterus at the labia, and often had great difficulty in replacing it in the vagina; the prolapsed tumour was pyriform, and the fundus presented a smooth globular surface, bleeding on the slightest touch, and the

[87] Levret says, that in total inversion the tumour is pyriform; but in partial inversion, the fundus projecting through the os uteri is demi-spherical, and not pyriform. (*Cure des Polypes de la Matrice*, p. 130.)

[88] See description of Plate 6; also *Catalogue of the Museum of the College of Surgeons of Ireland*, by Dr. Houston, ii., 509; one of the best specimens of a work of this description in any language.

[89] *Bulletino della Scienze Mediche di Bologna*, xi., 124. When examined in the vagina some time previously, the tumour was compared to a pullet's egg; the above description refers to it when prolapsed:—"*Il tumore offiriva* la figura piriforme del volume de una piccola petronciana, *coll'apice alla vagina, e la base alle cosce, e la mucosa, che lo ricopriva, vestita di placche cancrenate.*"

[90] *London Medical Gazette*, xxv., 151.

summit or neck was surrounded by a reflection of mucous membrane answering to the os uteri. There is a similar case by V. Horn,[91] with much greater disease of the inverted organ; the prolapse took place at the expiration of five years from parturition.

When the uterus prolapses at the labia, the vagina necessarily becomes inverted; and as the uterus descends, the vagina is brought down with it, and appears externally, forming a hollow tube lined with the peritoneum, thin, soft, and yielding, compared with the uterus; the bladder, and also the rectum, may be brought down with the vagina,[92] and the bowels may also enter the vaginal pouch. Even the whole of the vagina may be thus brought externally, inverted, and terminating above at the labia. La Motte[93] gives two examples, in one of which the prolapse was a foot in length. When the inverted uterus is external, the menstrual loss has often been observed to flow from its surface; I have been informed of a maniac afflicted with this infirmity, and in whom the menstrual phenomenon was often witnessed. After the menstrual period of life is past, if the inverted uterus prolapse constantly, its surface assumes the appearance of the neighbouring

[91] The uterine tumour increased during the five years, from chronic irritation and inflammation, till distending the vagina it impeded the evacuation both of rectum and bladder; and when at length it prolapsed externally, from the supporting bandage being laid aside during efforts at defecation, the tumour was found scirrhous, as large as a child's head, and shortly caused death by becoming gangrened. (*Schmucker's Vermisch. Chir. Schriften*, ii. 143.)

[92] I believe it rarely happens that either bladder or rectum are brought down below the labia, although this is regarded by Levret as unavoidable and characteristic in total prolapse of the inverted vagnia and uterus, and is supported by an autopsical dissection hereafter to be noticed:—"La descente parvenue à ce degré, *entraîne nécessairement la vessie urinaire* et le vagin, de manière à faire ensemble un col creux à la tumeur, lequel est attaché circulairement à l'entrée de la vulve, qu'il bouche par continuité. (*Mém. de l'Acad. de Chir.*, 8vo. edition, iii. 470.)

[93] *First Case.*—In an old lady, the tumour hung big as one's fist between the lower limbs and had many inequalities; its surface was very dry and sensible of cold. The malady commenced after delivery, increased gradually, and had been present for thirty years... *Second Case.*—The tumour, consisting of the inverted vagina and uterus, hung out between the limbs to a foot's length, and at the upper or vaginal part it was no bigger than a child's arm in circumference, continuous with the external labia, but leaving the urethra free: thus it continued for three fingers' length, and terminated below in a large body, the uterus, resembling a *gourd*. It came on many years before, consequent upon a lying-in, and by neglect of pessary and bandage arrived at its present state. This patient appeared in good health, as if she might live a long time; but the other patient died soon after La Motte saw her. (*Traité des Accouchemens*, pp. 653–6.)

cutis; and the same change happens to the inverted vagina, from continued contact of the atmosphere, at any period of life. In all such cases, although the patient, being possessed of a strong frame and accustomed to active bodily exercise, may move about, she is a miserable, loathsome object, walking with knees apart, suffering pain and discomfiture from ulceration, inflammation, and even sphacelation, of the surfaces of the prolapsed parts, chiefly the result of urinary incontinence, which invariably attends every extreme case of prolapse.

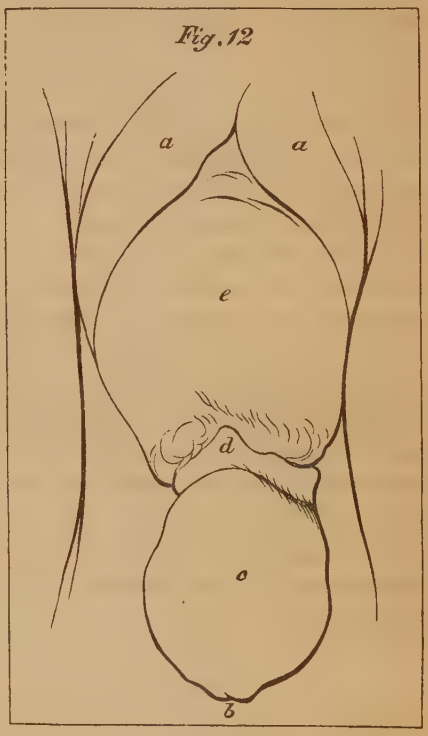


Fig. 12

I cannot conclude the subject with a more remarkable example than the one where Mr. Chevalier removed the prolapsed and inverted uterus, and of which Fig. 12[94] is a correct representation, half the size of the original drawing. The patient was fifty-four years of age when the sketch was taken just before the operation, and had suffered from the disease many years: she had continual *stillicidium urinæ*. The uterine surface furnished a constant mucous discharge, and the inverted vagina, greatly thickened, was of an opaque whitish colour, and had a leathery feel. No doubt the bladder was drawn down in some degree by the vagina, and not only the uterine appendages, but portions of the bowels,

[94] In Fig. 12 (*a*) marks the labia; (*b*) the inverted fundus; (*c*) the body of the uterus inverted; (*d*) the cervix not inverted; (*e*) the vagina inverted and exposed to the external air, forming a large bag, lined with peritoneum and communicating with the abdominal cavity. Mr. Chevalier observes:—"Adhesive inflammation must have taken place and have fixed the parts permanently in their unnatural situation; and it will be evident from the drawing, that *the fundus* of the bladder *must have been drawn down* into the prolapsed vagina."—*Merriman's Synopsis*, p. 307.

descended into the spacious vaginal bag, which freely communicated with the abdomen.

Under the worst symptoms, whether the inverted organ be prolapsed or still resting in the vagina, the patient[95] is rendered bloodless and emaciated to the utmost, from profuse losses, sanguineous, leucorrheal, or purulent; the spasmodic pains and interruption to the function of the contiguous viscera add to the evil results; the lower extremities become dropsical, and the patient sinks under the continuance of hectic symptoms, not unusually the silent victim of her concealed or misapprehended malady.

SECTION IV.

ON INVERSION OF THE UTERUS FROM POLYPUS.

Next to pregnancy, the most frequent cause of enlargement of the uterus is a polypous tumour, which when attached, as often happens, to the internal fundus of the organ, may occasion its inversion in all the different degrees that have been referred to. Any of the various tumours[96] that progress towards the uterine cavity, and take the polypoid form, may induce inversion; but the vesicular polypus, being softer and of less density than others, and having usually a narrow neck, is less likely, in the progress of its growth and of its expulsion, to cause uterine inversion, whilst the polypus of great density, and with a broad basis, and particularly the fibrous, is not unfrequently followed in its latter stages by the displacement in question.

[95] "Des spasmes hystériques troublent l'économie générale dans ses fonctions, la peau s'étiole, et la fièvre lente hectique termine plus ou moins promptement sa déplorable existence." (Martin-le-jeune, *Mém. de Méd.*, p. 226.)

[96] Polypous or pediculated tumours, whose descriptive character is "to be narrowest at their root or attachment to the interior surface of the uterus," are of various kinds, and require to be distinguished in reference to their liability to cause *inversio uteri*:—1st. The *vesicular* polypus, resembling those of the rectum, bladder, or nostril, and arising from the lining mucous membrane of the uterus; smooth, pyriform, often with a very narrow neck, soft, not of great specific gravity: it is pediculated from a very early period after its origin, does not attain a large size, and is so frequent that many surgeons, trusting to their own small experience, know of no other.—2nd. The *fibrous*, very different in its origin, history, and character. A fibrous tumour may arise in any of the cellular texture of the uterus, whether just beneath the peritoneum,

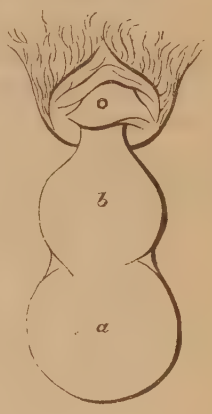
One of the earliest records of this displacement is that so graphically described by Von Sanden,[97] accompanied by a repre-

between the muscular fibres, or next the mucous lining of the uterine cavity. When originating in this last situation, the tumour bulges into the uterine cavity, covered by the mucous membrane, and attains a considerable size before it takes the polypous or pediculated form; and even when it has undergone this change, the basis or neck, by which it is attached to the uterus, is large, unless the os be dilated, or the tumour, propelled from the uterus, lodges in the vagina, when the os, constricting its basis, narrows it into a neck, and gives the pyriform shape. The fibrous polypus is firm, dense, of great specific gravity, in shape approaching to oval, somewhat nodulated or irregular in its surface; it may attain to an enormous size, without any decay, and is covered with the lining mucous membrane of the uterus, which has been pushed before it and still remains a distinct covering, from which the tumour is enucleable, even at an advanced period of its growth. Dupuytren, and other writers best acquainted with the subject, affirm that the fibrous are the most frequently met with of all polypous tumours.—3rd. The *fungoid* or medullary polypous, different from either of the preceding, in being malignant and not curable by removal; always soft, and not necessarily pediculated at an early stage, but always becoming so when protruding into the vagina through the os tincæ; very vascular, bleeding profusely, and lacerated by the slightest touch; rapid in growth above all other polypi, and involving the mucous membrane of the uterus, so as to have no covering separable from itself. I name these hastily, as the three great distinctions of polypus growths to be kept in mind in this Essay; each class has subdivisions, which cannot here be dwelt upon.—4th. There are other uterine tumours assuming the polypoid shape, and of rare occurrence; some cellular and containing fluid, others a mere hypertrophy of the actual texture of the uterus itself projecting into its cavity, as demonstrated by Cruveilhier. Of hollow polypi, as described by some writers, I have doubts, save such as may consist of a simple cyst filled with fluid, and as resemble the tumours called cysts, not unfrequently met with in other parts of the body. It has been ascertained that polypi grow faster during pregnancy; they may also adhere to each other by the inflamed or ulcerated surfaces coming in contact, or to the uterus under the same conditions. The instance of a polypus of the rectum, and one arising in the vagina, becoming adherent together, is amongst the curiosities of pathology upon this subject.

[97] *Observatio de Prolapsu Uteri Inversi ab Excrescentia carneo-fungosa, in fundo ejus interno enata, &c.* 4to., Regiomonti, 1722; p. 9.—"Mulier quædam ætatis suæ triginta sex annorum, ante annos sedecim filiam peperit, ante tres vero annos sutori nupsit, a quo quoties imprægnata, toties abortum passa est, subsequente semper enormi hæmorrhagia uteri, et quovis mense debita menstruatione interveniente. Hæc pertæsa abortuum sibi molestorum ab affine suo medicamen aliquod contra impræg-nationem petiit; ne hisce hæmorrhagiis, ex illa oriundis, amplius subjiceretur. Affinis precibus annuens propinat illi infusum aliquod... Usum hujus infusi optatus, ut hæmorrhagia uteri sisteretur, effectus est secutus, alvus vero exinde ita constipata, ut, cum eandem summis nisubus in area supra terram deponeret, loco fæcum alvinarum corpus pugni instar expresserit, quique nisus illius vires adeo prostraverant, ut dorso incurvato hypocaustum repetere, seque ad lectum conferre cogeretur, ubi illud, quod præter opinionem exciderat, in locum priorem ipsa reponere alloboravit, verum

sentation of the appearances presented by the depending tumour, and totally inverted and prolapsed uterus, of the natural dimensions, and which is here presented in outline Fig. 13, diminished to one-sixteenth of the original size; (*a*) is the tumour, attached to the entire fundus of the completely inverted uterus (*b*), which tapers towards its neck, and in shape strictly resembles the Florence flask; the vagina is also brought into view, inverted, and the *ostium urethræ* is visible, clear of the external labia; a slight narrowing marks the boundary between the uterus and the polypus, but the two are blended together by similarity as well as continuity of surface, and approach the shape of an hour glass. The surfaces of the depending mass were sphacelated[98]

Fig. 13



frustra... Non multo post hæmorrhagia uteri rediit, quam comitabatur fluor albus, quæ bina symptomata repositionem uteri prolapsi non permisere... Chirurgus uteri prolapsum esse judicavit, qui vero adeo a sphacelo jam fuit corruptus et exulceratus, ut cadaverosum de se spargeret odorem. Hunc itaque adhibitis fomentis, epithematis atque injectionibns propellere, et uterum prolapsum reponere, fasciisque commodis detinere, sed sine effectu, studuerunt; quoties enim alvum deposuit, toties iterum uterus exiit, huic malo superveniente incontinentia urinæ.. Excrescentiam carneam ex uteri vagina natam existimantes, ligatura eandem esse auferendam judicarunt, illamque applicarunt, quam vero post dimidium horæ, ob insecutas convulsiones, laxare debuerunt. Resectionem autem non tuto hic posse adhiberi, colligebant ob metuendam hæmorrhagiam vix compescibilem, multis enim vasis sanguifluis gaudebat, et multum sanguinis, cum tangeretur, et a pure putrido purgaretur, carnea hæc moles fundebat."

[98] Called after the first ligature had been applied, Von Sanden describes the sequel of the case:—"Facta religatione apparuit mihi pendulum extra abdomen corpus illa facie atque magnitudine, quam figura exhibet, totum sphacelatum, nigrum, multo pure fœtidissimo scatens, et hinc inde per lamellas decorticari incipiens. Cogitabam vero statim, corpus illud carnosum esse uterum prolapsum, eumque ab excrescentia carnea inversum, cum nullum orificium uteri internum oculis vel stylo exploratorio observare possim. Hujus rei ut certior fierem, explorabam intra vaginam uteri totam, num uterum ibidem cum suo orificio interno reperire possem, sed nihil

here and there; and after a ligature had been twice applied, and as often removed on account of the severe symptoms—convulsions and spasmodic pains—the patient died. A rumour prevailing that the surgeons had occasioned death, authority was given for an autopsy, at which Von Sanden attended; and in the work from which I am quoting he gives a second plate, representing the appearances observed in the abdomen, the ligaments, both round and large, and the Fallopian tubes, being in great part drawn into the peritoneal pouch of the inverted organ, and the ovaria resting at the entrance to this pouch.

In thus referring first to the total displacement of the uterus from polypus, I am quoting historically; but inversion from polypus has been met with, in all the minor degrees, both by the practised surgeon, and by the anatomist on dissection. If the polypus be attached to any part of the fundus, at the terminating opening of either of the Fallopian tubes, or in the interspace between those openings, it may determine a partial and limited inversion, whilst still remaining wholly or chiefly within the uterus. The symptoms are the same as a polypus produces without inversion, and cannot be considered characteristic, although generally more severe, such as uneasiness in the uterine region, forcing pains, leucorrhœa, and menorrhagia. An illustrative example has recently been published by Dr. Oldham,[99] with a plate, of which I have given an outline in Fig. 14, reduced to one-half the original size. The tumour was attached internally to the right horn of the uterus, where the Fallopian tube terminates; and at this spot the inversion took place, and by degrees projected considerably into the uterine cavity, forming the pedicle of the tumour; (*a*) points to the outer surface of the tumour, which has been divided; (*bb*)

mihi inquirenti obvenit... Licet hæc moles carnea per decorticationem indies a medicamentis externis decresceret, lente tamen hæc ipsa fiebat, ut iterum ligaturam Chirurgi meditarentur, et me præsente et approbante applicarent: sed nec secunda vice tentatam, ob dolores spasmaticos exinde ortos, perferre potuit. Supremum obiit diem 22 Martii ann. 1719." (*Ibid*, p. 11.)

[99] *On Polypus Uteri and its co-existence with Pregnancy, in Guy's Hospital Reports.* New Series, ii., 105, 136.—The patient, a virgin lady, between fifty and sixty years of age, had long suffered from pain, hæmorrhage, and offensive discharge. The disease was thought to be malignant; and, refusing to submit to a local examination, she died, worn out with the discharge and bleedings." The polypus was firm, compact, and fibrous; and its lower free surface, which had passed beyond the os, having freely dilated it, was sloughy and ulcerating.

rest on the two cut surfaces, the greater portion of the tumour being lodged in the uterus (*cc*), and the rest presenting at the dilated os; (*d*) is the right horn of the uterus inverted, into which the tumour insensibly merges; (*e*) points to the opening of the right Fallopian tube, stretched and patent in front of the inverted portion of the uterus; the right ovarium (*f*) is close to, and the Fallopian tube (*g*) is partly lodged in, the cavity formed by the inversion, whilst the left Fallopian tube (*h*) and its ovarium (*i*) remain *in situ*. In this instance it was easy to detect the polypous tumour, as it presented at the os tincæ, and its size would be indicated by the increased size of the uterus; but of the inversion

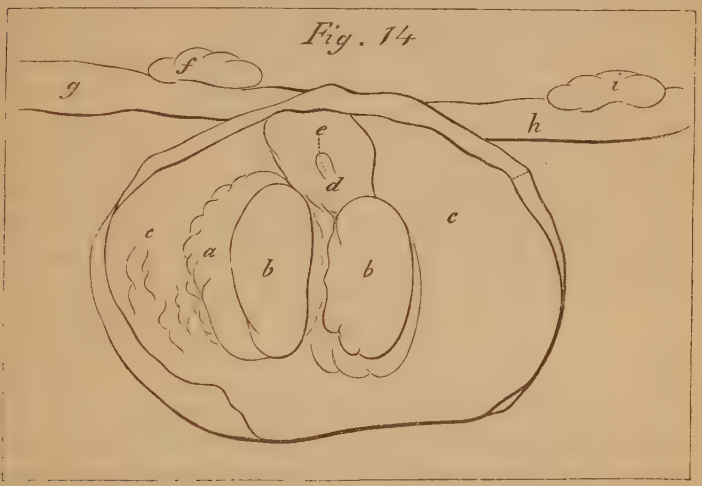


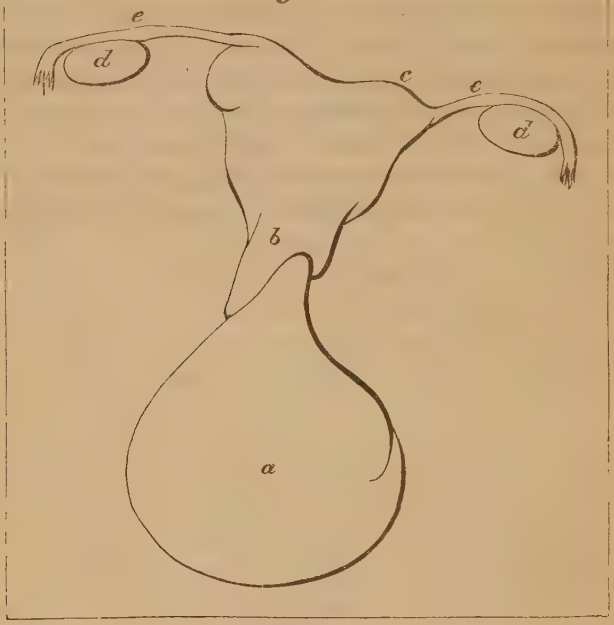
Fig. 14

no evidence could be gained, unless the irregularity of shape at the left horn of the organ could be ascertained either from examination by the rectum, or through the abdominal parietes above the pelvis. A ligature applied to remove the polypus would inevitably have included part of the uterine inversion.

A polypus may, however, occasion deformity of the uterus, without inversion, after the manner exhibited in a preparation which Dr. Bull has favoured me with a sight of, and of which I give a miniature outline in Fig. 15: a huge polypus (*a*) attached to the anterior and left part of the os tincæ (*b*) and contiguous part of the uterus, having depressed the left horn (*c*) without inversion, and thereby placed the fundus obliquely, and destroyed all

symmetry between the ovaria (*dd*) and Fallopian tubes (*ee*) of each side.[100]

Fig. 15

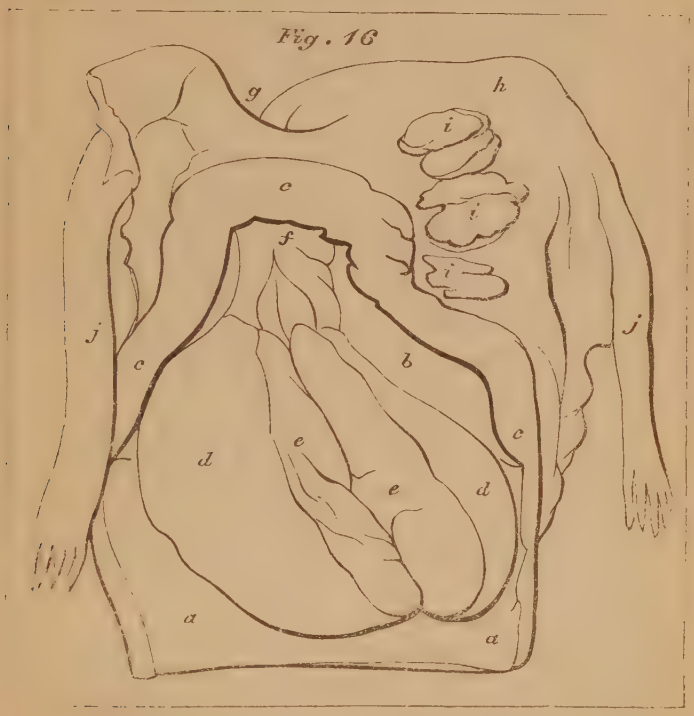


In the Museum at St. Bartholomew's Hospital there is an example of a large fibrous polypus, with a portion of the inverted fundus of the uterus near its right horn for its pedicle; nearly half the polypus has passed through the dilated os and is lodged in the vagina, the rest still occupying the uterine cavity. Some idea of this preparation will be conveyed by the outline Fig. 16;[101]

[100] "Les polypes peuvent abaisser l'organe gestateur, et lui faire éprouver des déviations." (Lisfranc, *Clinique Chir.*, iii. 76.)

[101] In Fig. 16, which is about half the size of the preparation to which it refers, (*a a*) mark the interior surface of the vagina; (*b*) is the cavity of the uterus; (*c c c*) its walls divided; the polypus (*d d*) lodges partly in the uterus, and partly in the vagina; (*e e*) are the cut surfaces of the polypus, which is thus laid open to show its firm and fibrous structure; (*f*) the neck of the polypus, formed by a narrow but lengthened inversion of the right horn of the uterus, and causing a depression at (*g*), where is situated the entrance into the peritoneal pouch formed by the inversion, and which pouch extends nearly to the polypus, below the letter (*f*); the left horn of the uterus (*h*) is *in situ*; (*i i i*) are small fibrous tumours, cut into and expanded; (*j j*) the Fallopian tubes.

and it will be evident that the further expulsion of the polypus from the uterus into the vagina would necessarily be followed by an increase of the inversion, until the fundus approached the os tincæ, and the whole of the polypus rested in the vagina.



A polypus is most likely to cause inversion when attached to some part of the internal fundus. Boyer[102] says that this is the most frequent seat of its attachment; the same has been asserted by Lisfranc,[103] who further states that, of sixty polypous tumours removed by him, forty-three were attached to the fundus, somewhere between the orifices of the Fallopian tubes. This able writer also states that the polypus with rather a large pedicle is most likely to cause inversion; the preceding examples, with many more which might be collated, prove that when attached to the fundus a small pedicle is sufficient; and Velpeau[104] has related a fatal case where the pedicle was only of the size of one's finger.

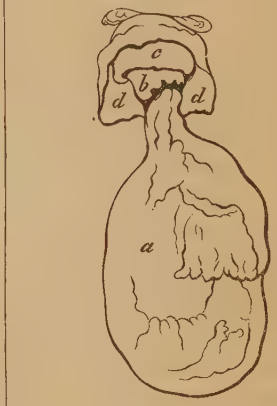
[102] *Traité de Mal. Chir.*, x., 555. [103] *Clinique Chir.*, iii. 86.
[104] *Méd. Opératoire*, iv., 383.

The polypus may pass gradually through the dilated os uteri, and lodge entirely in the vagina, giving rise to a gradual increase of the inversion; but more frequently the expulsion of the polypus is quickly effected under severe uterine contractions and expulsive pains, like those of parturition, and the inversion is in such instances rapidly produced or increased. Where the uterus had become enlarged,[105] so as to be felt three inches above the os pubis, presenting certain elevations which were regarded as fibrous tumours, the os uteri was still closed; and at this stage of the malady violent expulsive pains came on, as well in the intervals of, as during menstruation; suddenly there was an abundant menorrhagia, accompanied by most atrocious pains, like those of labour; the patient died in a few hours. On autopsy a fibrous tumour was found in the vagina, attached to the fundus by a pedicle of considerable size, and the uterus, otherwise healthy, was completely inverted.

It is, however, so usual for polypi to be expelled into the vagina by violent uterine contractions, that we must not expect in all such cases to find uterine inversion; but before coming to a conclusion we should institute such investigations by the touch, as will be pointed out fully in considering the diagnosis and treatment.

The inversion may be first induced, or after its commencement be carried to a greater extent, by the polypus, after lodging in the vagina, being expelled thence at the labia. The example, of which Denman has given so elegant a Plate,[106] may be quoted:—The woman had suffered from uterine disease for three years, and concealed it; the polypus rested in the vagina, till at length the pains, like those of labour, were renewed, and it was excluded at the external labia; death ensued in a few hours. The tumour, soft and spongy, weighed one pound four ounces. The fundus uteri was inverted and dragged through the os, having the relations

Fig. 17



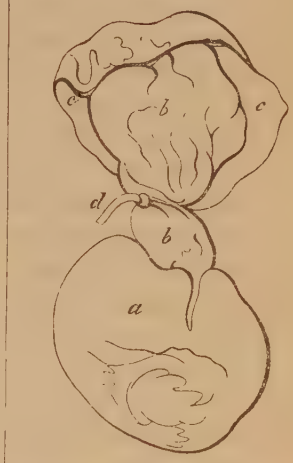
[105] Lisfranc, *Clin. Chir.*, iii., 77–78.

[106] *Practice of Midw.*, 4to., Plate 14.

shown in the miniature outline Fig. 17, one-sixteenth of the original plate in size, in which (*a*) is the tumour, separated by an oblique and irregular line at its neck or pedicle, from the fundus (*b*), which is inverted and brought down so as to be visible below the os (*c*), and to rest in (*dd*) the vagina.

Denman, one of the most candid of authors, has furnished us with another illustration of inversion from polypus, which was of large size and fibrous, growing without any stem, and reaching the vagina by bringing down the inverted fundus, to which it was attached, till it passed through the os uteri and proceeded to the third degree of partial inversion, as shown in Fig. 18.[107] This outline is one-sixteenth of the original Plate in size; (*a*) marks the polypus; (*bb*) the inverted fundus and body of the uterus below the os; (*cc*) the interior of the vagina, the cervix being concealed and the uterine appendages not represented. For the removal of this polypus a ligature was placed just above it, upon the inverted fundus of the uterus (*d*), and proved fatal in four or five days.

Fig. 18



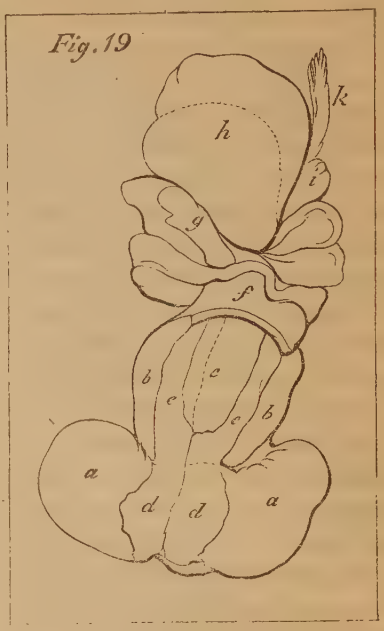
To trace further the progress of a polypous tumour with inversion, I may refer advantageously to the case published by Dr. D. D. Davis.[108] The patient, an aged woman, was brought to the Middlesex Hospital in a dying state, and an autopsy demonstrated the condition of parts, as represented in Fig. 19,[109] which is one-

[107] *Practice of Midwifery*, 4to., Plate 17.

[108] *Principles of Obstetric Medicine*, i., 618, Plate 21.—The patient was brought to the Middlesex Hospital in a dying condition; the polypous tumour prolapsed beyond the external labia.

[109] This preparation is now preserved in University College Museum, London; it is unfavourably displayed, the sections being made neither anteriorly nor laterally, but between the two; still much more may be learnt from an inspection of it than

sixteenth of the original size. The large polypus (*a a*) protruded at the external labia, and was eight or ten inches in its largest diameter; attached extensively to the fundus uteri, it had caused total inversion of this organ, which occupied the vagina, immediately within the labia; the outer surface of the inverted uterus, which was its internal lining, is marked (*b b*); both the tumour and the inverted portion of the uterus have been partially divided, showing them to be continuous; (*d d*) the divided surfaces of the tumour; (*e e*) the same of the uterus; (*c*) is the peritoneal pouch formed by the inversion, and communicating with the general abdominal cavity; the dotted line showing the situation and course of one of the Fallopian tubes, which in the preparation is laid open by an incision first carried through the peritoneum, and a bristle is introduced into its cavity; (*f*) is a part of the vagina, concealing the cervix uteri; (*g*) lateral view of the divided os pubis;



the original drawing could possibly convey. Calculating from the *ostium urethræ*, about one inch of the vagina remains uninverted, so that the angle of its reflection, where the circular cul-de-sac terminates, is situated at the depth of an inch, from which the inverted vagina extends downwards two inches, forming a tube, which terminates in the uterus, at which termination there is a distinct thickening or circular prominence, answering to the cervix uteri completely inverted. The cut surface of the inverted uterus is nearly an inch in thickness, and the peritoneal pouch or diverticulum formed by it is very small, as represented in the plate, and its whole extent is laid open. There is no observable alteration in the bladder from its normal position, the superior fundus rising prominently towards the abdomen; and in the posterior view, the ovaria are of large size, and lie close to each other at the margin of the peritoneal pouch, tucked in behind the bladder. The peritoneal pouch, though narrow, must be about four inches in length, two inches answering to the inverted vagina, and the rest to the uterus totally inverted. The left corpus fimbriatum is adherent, the right loose and floating.

(*h*) the urinary bladder, distended and in situ, the surface above the dotted line being covered by the peritoneum; (*i*) left ovarium; (*k*) left Fallopian tube and corpus fimbriatum.

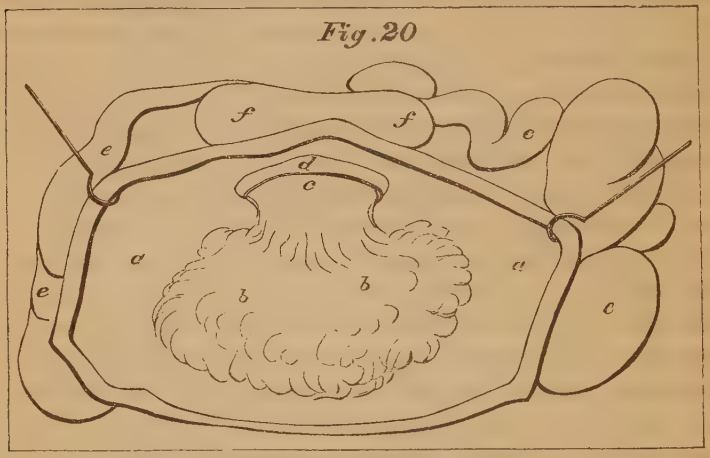
Thus have I mapped as it were the habitation of a polypus inducing inversion, at first so partial that the polypus is still situated in the uterus, and next it descends into the vagina, bringing the inverted fundus to the os; then the polypus protrudes at the labia, the displacement being carried to the greatest degree of partial inversion and filling the vagina, whilst the cervix alone continues *in situ* or is itself inverted with part of the vagina. A further stage remains to be demonstrated, in which the uterine inversion is rendered *total*, and prolapses externally, bringing with it the vagina, also inverted. I am able to accomplish this purpose by referring to a still more striking case than any of the preceding, and which has hitherto been only partially made public. The preparation to which Plate 8 refers[110] is preserved in the Museum of the Royal College of Surgeons in Dublin, and was prepared and displayed, so as to answer precisely to the drawing, by the able hand of Dr. Houston. The inversion of the uterus is total, and its prolapse complete; the vagina is likewise inverted and prolapsed, following the uterus, and it is greatly thickened; whilst the cause of all this displacement was a small polypus, not much larger than a chesnut, nor very unlike one, situated inferiorly and attached to the very centre of the inverted fundus of the uterus. The first symptoms experienced by this patient were constant bearing-down pains, with a sense of weakness in the pelvic region, and an inability to move about or to make any trifling exertion; these were after some time followed by profuse leucorrhœa, occasional menorrhagia, and great irritability of the bladder. The tumour, after having been somewhile lodged in the vagina, gradually passed beyond that canal and formed a considerable protrusion externally; neither pressure nor the recumbent posture had much effect in reducing its size. She suffered for four years before she applied for relief at the Meath Hospital, and died in a short time after admission, exhausted by the continued irritation of the disease.

The vascular or fungoid tumour, which rarely takes the polypoid form until it presents in the vagina, and is constricted by the cervix

[110] See Plate 8 and description; also Dr. Houston's *Catalogue of the Museum of the College of Surgeons of Ireland*, p. 508, F. c., 690.

uteri, may cause uterine inversion in its progress, particularly if centrally attached to the internal fundus, between the orifices of the Fallopian tubes. In a case of this kind, where the tumour protruded externally, and displaced the uterus to the greatest degree of partial inversion, Rheineck[111] successfully removed chief part of the inverted organ, together with the tumour, by ligature.

Fig. 20



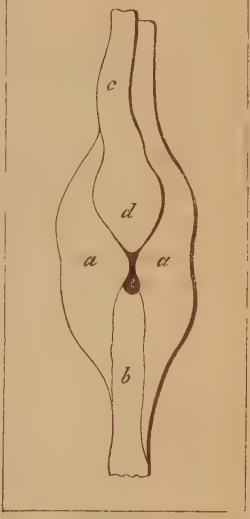
Boivin and Dugès[112] have given a representation of a case of vascular or encephaloid tumour, which lodged in the vagina, attached to the entire surface of the fundus uteri, which was brought down through the os and cervix: Fig. 20 will convey an idea of the displacement, and is half the size of the original plate.

[111] "Beobachtung und glückliche Heilung einer vollkömmen invertirten, mit einem Fungus hæmatodes behafteten und brandig gewordenen Gebärmutter, mittelst der Unterbindung," in *Siebold's Journal der Geburtshülfe*, v., 628.—The patient had never been pregnant, and was forty-one years of age when she presented herself to the operator, greatly weakened and almost hectic from loss of blood. Her symptoms had lasted only a few months; and after severe uterine pains, a tumour prolapsed externally, being the polypus with the inverted uterus, now a dark, dry, and sphacelated mass. The uterine inversion was complete, no ring or fold encircling the part next the vagina, answering to the cervix, and which Rheineck states to be always the case when polypus is the cause of complete inversion. Examining per vaginam, he felt on the left side either the round ligament or Fallopian tube. The neck of the prolapsed mass, formed by the inverted vagina, was of the size of an infant's arm, soft, hollow, and yielding to the touch. The ligature separated on the seventh day, detaching the inverted uterus and with it the tumour; the patient was well a few weeks afterwards. A rough drawing is given of the semi-disorganized mass removed by this operation.

[112] *Maladies de l'Utérus*, Plate 35, Fig. 2.

In the vagina (*a a*) lodges the entire mass of malignant soft tumour (*b b*), resembling a cauliflower; it is largely attached at its root to the inverted fundus of the womb (*c*), which has descended into the vagina, and is encircled by the dilated cervix (*d*); the Fallopian tubes (*e e e e*) are seen greatly enlarged, dropsical, and convoluted; (*f f*) is the uninverted portion of the body of the womb. The dip of the fundus in this case, as viewed from the abdomen, is shown in Fig. 21[113] (*a a*); resting on the body of the uterus at the angle of reflexion, and between those letters, is the depression leading to the peritoneal cul-de-sac, a funnel-like cavity capable of admitting the finger; one Fallopian tube (*b*), although enlarged, is otherwise healthy; but the other (*c*) has a small cancerous tumour (*d*) attached to it and obstructing it, proving how unavailing, as regards a permanent cure, would have been the removal of the tumour in the vagina. Within a few years I met with a case of fungoid disease causing uterine inversion; the patient was an aged maiden lady, residing at a distance, to whom I was urgently summoned on account of hæmorrhage from the vagina, so profuse as to endanger life. I recognised the malignant tumour in the vagina, and was induced to apply a ligature, as I have more than once done on other occasions of the same pressing description, not in the hope of curing, but of arresting the alarming hæmorrhage; I placed the ligature within the os uteri, upon the firmer as well as narrower part of the tumour; it occasioned no immediate pain, but within an hour such severe symptoms arose—pain, vomiting, and syncope—as convinced me that the inverted fundus of the womb was included, and at the end of two or three hours I was compelled to remove the ligature, having previously loosened it without giving sufficient relief. The distressing symptoms speedily subsided; the hæmorrhage was also

Fig. 21.



[113] *Maladies de l' Utérus*, Plate 35, Fig. 3.

arrested by the temporary application of the ligature, and did not return for some weeks, so that the patient more slowly fell a victim to her incurable malady.

Since pregnancy may exist in conjunction with polypous tumours of the uterus, it results that uterine inversion may occur under this complication. Where a polypous tumour is attached to the fundus, and the placenta is engrafted upon the polypus, as has happened, inversion of the uterus in the delivery would seem to be almost inevitable.

SECTION V.

INVERSION OF THE WOMB FROM THE REMOVAL OF POLYPOUS AND OTHER TUMOURS BY OPERATION.

The ligature was first employed, for the removal of uterine polypi, by Ambrose Paré, who used it only when the tumour was prolapsed externally, and applied it on a level with the external labia, or just within the vagina. Levret made a great advance in this line of practice, by effecting the removal of polypous tumours still retained in the vagina, the ligature being put around their pedicle at the os tincæ, and no more of the diseased mass removed than had been expelled from the uterus. We are indebted to the bold and original practice of Herbiniaux, for undertaking to remove polypi while yet lodged wholly or in great part in the uterus. After repeated and unsuccessful attempts to remove by ligature a large fibrous polypus,[114] which projected through the *os uteri* into the vagina, he imagined the method of producing inversion of the uterus, by drawing the polypus forcibly downwards, which he thought might be done with safety, from the circumstance of his having known the accidental occurrence of inversion after delivery terminate without a fatal result, the organ being speedily reduced. The polypus was accordingly seized with forceps, constructed after the manner of those used in midwifery, each blade being separately introduced, and by traction he brought down the polypus to the external labia: this was done thrice, at short intervals, before the operator could make any part of the polypus appear external to

[114] *Traité sur divers Accouchemens, et sur les Polypes de la Matrice*, ii., 109.

the labia, which was at length, however, accomplished, without producing more than a partial inversion of the womb. The pedicle of the tumour was thus rendered visible, and a ligature being placed in the furrow caused by the former ligature, and fastened, the polypus was cut off a finger's breadth below it, and instantly the inverted fundus uteri disappeared, being drawn up and concealed within the vagina, taking the ligature with it. Threatening symptoms soon ensued, which compelled the operator to brave the danger of hæmorrhage from the stump or cut surface formed by the operation, and to loosen the ligature a very little at a time. After six days the ligature was taken away; no hæmorrhage followed, but a quick and perfect recovery.

The name of Dessault has been associated by his countrymen[115] with that of Herbiniaux, for first suggesting traction in order to invert the uterus and to facilitate thereby the removal of a polypus; but the latter seems to me alone entitled to originality in the matter, and has an undoubted claim to priority in date.[116] In France the practice of inverting the womb in some degree, to facilitate the removal of polypi by excision, has been much in vogue; it is little followed in England, because the ligature is usually preferred, which can be applied whilst the polypus is still partially situated within the cavity of that organ. In the removal of very large polypi from the uterine cavity, the os and cervix

[115] Desormeaux, *Dict. de Méd.*, xviii., 270.—A woman, about forty-five years of age, was supposed to suffer from descent of the womb, and wore a pessary for ten years. Dessault being at length consulted, ascertained that there was a large uterine polypus, beginning to escape into the vagina; she was exhausted by menorrhagia, and had long kept to her bed. Dessault directed that she should rise and walk about her room, which she did the same day, and was only just up and moving about when the polypus made its escape, bringing with it the fundus uteri, and causing complete inversion, not of the uterus only, but of part of the vagina. The tumour was so large, and the patient in so exhausted a state, that Baudelocque deemed all attempts to replace the former in the vagina useless; and Dessault, on arriving after a few hours, passed a seton-needle through the pedicle of this enormous tumour, and conducted two ligatures, which were applied one to each half; these ligatures were placed on the inverted uterus. The tumour was next day cut off below them, and weighed five pounds, and immediately the stump retired within the os uteri, where it formed adhesions and remained permanently after the cure, so as to be felt presenting like an acorn in its cup (*sous forme de gland dans son châton*). (Dailliez, *Thèse, Obs.* xix., p. 60; from *Journal de Méd.*, lxxii., Août, 1787.)

[116] Herbiniaux's first case occurred in 1769, and Dessault's, as quoted in the preceding note, in 1787.

uteri have sometimes been incised freely, which must have the effect of rendering inversion more liable to take place, and facilitate its proceeding to the greatest degree.

When large fibrous tumours, situated in their origin near to the internal surface of the uterus, do not increase in this direction, so as to become pediculated, but in the opposite direction, towards the abdominal cavity, they carry the walls of the uterus before them, and these walls become greatly extended, and also thinned: such tumours, even when of immense size, if they present at the os uteri, may be removed, and in the course of the operation the uterus can scarcely fail to become inverted, and may descend into the vagina in this state, or prolapse at the external labia. Scoutteten[117] relates a remarkable case of this kind; and still more important are two cases recently recorded by Amussat, in which the tumours implicated more decidedly the walls of the uterus.

In Amussat's first case,[118] the fibrous tumour removed weighed about eleven ounces, and the operation lasted two hours. The tumour, presenting at the os uteri, was first exposed by cutting through the lining membrane of the uterus, which covered it, and was then gradually and with great difficulty enucleated and pulled downwards. When it was nearly detached, and about to be expelled, the patient was cautioned to avoid expulsive efforts, from the fear of too sudden an inversion of the uterus; the operator

[117] The patient was forty years of age and the mother of several children. After profuse hæmorrhage, and pains like those of labour, a tumour of great size was found presenting at the *os tincæ*. During two days, horrible pains continued; and on a consultation being held, the tumour was felt in the vagina, hard, resisting, smooth on its surface, with all the characters of a fibrous tumour; the abdomen was painful and tender to the touch. In the midst of these dangers an operation was determined upon; and on the tumour being seized and pulled downwards, it was found to adhere by a large surface to the uterus, which became inverted, and could not be replaced. A ligature was put firmly round the neck of the tumour, which immediately assumed a darker colour from obstruction to the circulation of blood in its numerous vessels. On the third day, the tumour was cut through below the ligature and removed, and the inverted uterus was gently replaced, with relief to all the symptoms, and a cure in less than a fortnight. The tumour weighed about thirty-six ounces, (*onze cents grammes*,) and was oval-shaped, its greatest circumference measuring above sixteen inches, and around the centre of the oval above twelve inches. (*Encyclographie des Sciences Médicales*, 3 ième Série, ix, 269; from *Gazette Méd. de Paris*, Août, 1839.)

[118] *Mémoire des Tumeurs fibreuses de l'Utérus*, p. 15; also Troussel, in *Revue Méd.* du mois d'Août, 1840; and *Encyclographie des Sc. Méd.*, 4 ième Série, i., 296.

also found the necessity of being most careful, for so thin were the walls of the uterus at one part, that the slightest mistake might have led to a wound of the peritoneum, and a dangerous if not fatal result. As soon as the tumour was quite delivered at the os externum, it was followed by the inverted fundus, which, however, did not pass through the os uteri, the latter contracting; and as the operator at the same time pushed back the inverted fundus with his fingers, it became replaced, and the patient recovered.

In Amussat's second case,[119] which was likewise successful, the fibrous tumour removed weighed about fourteen ounces, and the uterus was only slightly inverted, but sufficiently so to induce him to enjoin that, towards the conclusion of every such operation, where inversion is so liable to happen, you should avoid incisions and ligatures, and trust to the fingers for accomplishing the enucleation and final detachment of the tumour, lest you wound the peritoneal cul-de-sac formed in consequence of the uterine inversion.

Recently, at the Norfolk and Norwich Hospital, my colleague, Mr. J. Godwin Johnson, removed a fibrous tumour of enormous size, weighing thirty-two ounces; it is preserved in the pathological museum of the Hospital, to demonstrate the fact to any scientific observer. The patient sought advice on account of difficulty in emptying the rectum, and still more so in evacuating the bladder; indeed, retention of urine brought the nature of the disease first under our inspection. The tumour could be felt through the abdominal parietes, at least for six inches in length above the brim of the pelvis and towards the right iliac region; and in the vagina, it presented a large convex surface, implicating the os tincæ posteriorly and to the right side, so that the tumour must have been altogether nine or ten inches in length. Having been exposed, where it was accessible in the vagina, it was in part enucleated; and by successive trials day after day, more of the tumour was separated, chiefly by the fingers; at length the whole hand entered the uterine cavity and completed the separation of the tumour, which was removed by traction and by the aid of suitable instruments. When the last part of the tumour passed the external labia, the operator, who alone could be cognizant of this circum-

[119] *Mémoire des Tumeurs fibreuses*, p. 48; also Filhos, in *Revue Méd.*, Decembre, 1841; and *Encyclographie des Sc. Méd.*, 4 ième Série, ix., 63.

stance, felt the inverted portion of the uterus in the vagina, and judged its thickness to be not more than one-sixth of an inch. It was pushed up, so as to be in some degree replaced in the pelvis, and above the os; and although so thin when expanded, the uterine wall must have contracted, for Mr. Johnson assures me, that on examination after the patient's perfect recovery, there was not the slightest projection or irregularity observable, the uterus occupying the same space, and presenting the same shape, as in health. This heroic operation, not less remarkable for its successful issue than for many circumstances attending its execution, deserves to be speedily published in detail.

The result of operations of this description is always liable to be influenced by the presence of other fibrous tumours, for seldom do we find the tumour solitary; more commonly several are present, in different parts of the organ—sometimes as many as ten or a dozen, of different sizes. But I cannot dwell upon the subject here, as I permit myself to notice it only incidentally as a novel proceeding, and in conjunction with inversion of the womb, which is a constant attendant on almost every operation of the kind, and requires to be fully known, that danger may be avoided by the skill and judgment of the operator.

SECTION VI.

ON INVERSION OF THE UTERUS UNDER CIRCUMSTANCES NOT INCLUDED IN THE PRECEDING SECTIONS.

It is admitted that whatever distends the uterus and remains in it for a time, relaxing its parietes as well as enlarging its cavity, may, on expulsion, be followed by inversion of that organ. In addition to what has been already stated, we may, therefore, enumerate coagulated blood accumulated in the uterus, and an hydatidous growth or mole occupying its cavity, each of which has been known to cause uterine inversion. Distention of the uterus by air, or by aqueous fluid, has also been brought into this list; but no example of inversion from either of these sources has, so far as my researches extend, been satisfactorily recorded:

the statements relating to this part of the subject are conjectural, and based upon what is deemed probable—not upon the observation of actual occurrences.

The accumulation of blood in the uterus ranges next after pregnancy, and polypous or other tumours, as a disposing cause of inversion. How continually after delivery do we find the uterine organ re-distended, even to a great degree, with coagula! In every such instance that organ is placed under conditions rendering inversion possible, but in a very small proportion does the displacement happen. A few recorded cases might be quoted, and of these some are unsatisfactory; others evidently stated in error, the epoch of the inversion having manifested itself to the unsuspecting or uninstructed observer (perhaps some seven or ten days after delivery) not being the commencement of the displacement, which was coincident indeed with the termination of the labour. With more correctness may we regard distention of the uterus from blood as a cause of the *relapse* of inversion. A woman,[120] delivered at the full term, suffered inversion of the uterus by the removal of the placenta: the inversion, it is said, was reduced, and on the tenth day she felt severe pains, followed by abundant loss and a relapse of the uterine displacement. It has been conjectured[121] that blood had accumulated in the uterus, and that its sudden expulsion led to the relapse; but even in this instance we cannot dismiss the suspicion of the first reduction having been incomplete, a slight degree of inversion remaining, which at the end of nine days was rendered complete.

A woman, in whom the menses had been suppressed for three months, was supposed to be parturient: after pains had been experienced for four hours, there was considerable loss, and it was believed that the head of the fœtus descended to the external labia, where it remained, and the attendant was surprised to find it so large, as the patient was supposed to be only three months gone in pregnancy. In this difficulty Le Blanc[122] arrived, and found no fœtus, but a tumour in the vagina, equalling in size the fœtal head at six or seven months; he convinced himself that it was the uterus inverted, which he reduced according to art, and

[120] Sabatier, in *Mem. de l'Acad. de Chir.*, 8vo. edition, iii., 363.

[121] Ferrand-Demissols, *Thèse*, p. 9. For examples bearing on the same question, see foot-note No. 46, section II.

[122] *Précis d'Opérations*, i., 361.

the patient recovered. The idea has been suggested,[123] that Le Blanc mistook the case, and that it was simply a descent of the uterus. It seems quite as reasonable to regard it as an abortion, occurring after three months of utero-gestation, and giving rise to inversion, of which an instance in the experience of Lisfranc[124] has already been referred to in this Essay.

In all the preceding remarks and references, the accumulation of blood in the uterine cavity may be either affirmed or suspected to have been connected with pregnancy; and it will always be difficult, during the menstruating period of a female's life, to avoid all suspicion of this complication; the motives for concealment, or the ovum escaping unobserved, will tend to maintain the difficulty. Are there any cases, uncomplicated and authentic, of inversion, where blood was the primary cause of enlargement of the uterine cavity? I can not quote any so unobjectionable as the case related by Mr. Watkinson.[125] The woman is represented to have attained about fifty years of age, when the inversion took place during a protracted and very severe menorrhagia; the inverted organ prolapsed, and at the expiration of four or five years it hung pendulous half way towards the knees, with a neck formed by the inverted vagina about the size of one's wrist. The patient was reduced to imminent danger of life by sloughing and abscess, when the uterus was removed by incision below a ligature placed on the vagina, with a fatal result. No account, however, being given of an anatomical examination of the part excised, and the early history resting on obscure testimony, we cannot even in this case rely implicitly on the suggestion that menorrhagia solely, unconnected with pregnancy, led to the uterine inversion.

I am enabled to authenticate the production of inversion of the womb on expulsion of hydatidous masses, formed within its cavity, by the following original and interesting case, communicated by Dr. Thatcher, of Edinburgh, who has, for many successive years, detailed it to his numerous pupils, in his valuable Lectures on Midwifery, but not otherwise given it publicity:—

[123] Dailliez, *Thèse, Obs.* vii., p. 41.

[124] *Clinique Chir.* iii., 383.

[125] *London Medical and Physical Journal*, vii., 435. The explanation offered by this writer of the cause and origin of the inversion is, that in a relaxed state of the os uteri, and perhaps of the uterus itself, owing to protracted hæmorrhage, the organ became inverted on the expulsion of coagula, p. 436.

"The subject of this singular history had been a widow for seven years or more, when she sent for me in March, 1818, to relieve her from retention of urine. On examination I found the external parts covered with blood, which had been freely discharged from the vagina. I inquired if she had miscarried, or been delivered, both of which she denied; but requested me to examine what had passed into the chamber utensil, which I found nearly filled with an immense accumulation of hydatids, firmly cemented by nearly cartilaginous bands. In the vagina I traced a fleshy tumour, conoidal, about the size of the fœtal head at the seventh month of utero-gestation, and with every diagnostic mark of the inverted uterus, with the *os uteri* clear and defined, surrounding its upper base. There was also severe, dragging-down, dorsal pain, and exhaustion. The patient acknowledged that, being alone and unassisted, and finding this very unexpected mass protruding, she had used manual efforts for its extraction, and, in fact, she had produced the same consequences as would result from an injudicious and rude exertion with the umbilical cord, in removing the placenta after delivery at the full term of pregnancy. The distended bladder having been relieved by the catheter, persevering trials were made to re-invert the organ, and repeated at periods, for hours afterwards, until the latest visit at midnight; at this time the uterus was close down on the *os externum*. Next morning it was found fully protruded at the *vulva*, in shape and size like the largest caoutchouc bottle prepared for injection. Warm fomentations were used; afterwards an opiate ointment to cover the part; antimonials and opiates were also given freely, to relax, with the hope that reduction might still be effected; but all without success. On the following day, the prolapsed mass became more enlarged and congestive, with considerable exudation of sanious matter from its exterior surface; the *os uteri* was now, by careful examination, found so strongly compressing the tumour superiorly, as to interrupt the circulation in the mass below, and contributing to its separation by strangulation. It therefore became the duty of art to imitate nature; and as no portion of intestine could be traced in the peritoneal uterine cavity, a ligature of silver wire was applied close to the *os uteri*, with the double canula, as for a polypus. The ordinary means for supporting strength and preserving cleanliness were used. On the third day from the application of the ligature,

separation was nearly effected, and the slightly remanent portion was divided with the scalpel. The catamenia never returned, although the patient had previously, when in health, been regular."

I believe Dr. Thatcher's case to be unique. Hydatidous tumours[126] of the uterus have been justly named as by possibility giving rise to inversion of the containing organ; but I am not able to refer to any distinctly recorded case in which it happened; and there is certainly no other of the uterus, inverted from this cause, having been successfully removed.

Passing by the inadmissible statement made by Puzos,[127] in 1744, I find occasion only to note, in this section, certain histories, given upon the highest authority, where inversion of the womb was believed to have occurred in virgins, without the organ having been previously distended by any morbid growth. If we admit their correctness, we can only regard them as being of such rare occurrence as not materially to affect the general rule laid down in an early page of this Essay.

Baudelocque[128] relates that, in 1790, a young female, only fifteen years of age, who had for a long time suffered menorrhagia, consulted him when brought to a dangerous state of emaciation and debility. He found in the vagina, and above the hymeneal membrane, a tumour of the size of a small pullet's egg, oval, larger inferiorly, firm to the touch, like a healthy uterus, painful, and bleeding readily on pressure. The tumour was from one and a

[126] Excluding animal hydatids, which may sometimes be found in the cavity of the uterus, or in its walls, and referring only to the diseased masses not unfrequently occupying the uterine cavity, and denominated in France *la mole vésiculaire*, I was led to consider this a disease of the placenta, and an incidental result of pregnancy, by a case referred to in the *Transactions of the Provincial Medical and Surgical Association*, vol. iv., p. 303, and represented in Plate 6, the fœtus often dying in consequence of the diseased placenta, leaving this to increase indefinitely, both as to bulk and time. The same explanation had been previously offered by Boivin (*Recherches sur l'origine, la nature et le traitement de la mole vésiculaire*) and others. The firmness of the texture uniting the hydatidous cysts may be regarded as in some measure indicating the date of the disease; and in Dr. Thatcher's case, where the cysts were "firmly cemented by nearly cartilaginous bands," I consider it allowable to suppose that lawful pregnancy gave rise to the hydatidous disease, which had its seat in the placenta originally, although the patient had been a widow for seven years or more.

[127] The account given by Puzos may be read in Boyer, (*Traité des Mal. Chir.*, x., 488) and in numerous other authors; but it is so extravagant and unsatisfactory, that it should be allowed to pass into oblivion.

[128] Dailliez, *Thèse, Obs.*, vi., p. 39.

half to two inches long, and the vagina terminated at its pedicle in a circular cul-de-sac, no *os tincæ* being found. Baudelocque examined this patient very carefully twice at a short interval, and knew not the subsequent issue; but he regarded the tumour[129] to be an inverted uterus, from original malformation. Are we to suppose that Baudelocque, the most experienced of accoucheurs, and in this department particularly, was mistaken? Dubois has asserted this; and Lisfranc,[130] whilst he relies on Baudelocque's accuracy, suggests that retention of the menses, dropsy, hydatids, or even gas distending the uterus, might lead to an explanation of the inversion having taken place. Velpeau,[131] on the other hand, asserts that he has himself met with a case of inversion without pregnancy or other cause of uterine distension.

Let me conclude by referring to Boyer,[132] one of the ablest of surgeons and most perspicuous of authors, both pretty sure guarantees for his having been an accurate observer. He doubted the accuracy of the statement of Baudelocque, his contemporary, partaking of the prevailing doctrine as to the causes predisposing to inversion of the womb, and believing that such a displacement could not happen without the previous enlargement and distention of this organ, until he met with the inverted uterus in a woman who had not been pregnant for fifteen years, and in whom the uterus contained no foreign body: she was forty-four or forty-five years of age, had suffered no sanguineous loss beyond regular menstruation, no leucorrhœa, and had been the mother of three children. After experiencing weight in the pelvis, and a dragging in the loins, particularly when in the erect posture, she found a swelling presenting at the labia, which soon appeared externally; it was regarded as polypus, and the ligature was proposed for its removal. Boyer's attention to the case being now sought, he found the tumour prolapsing at the labia nearly an inch (*huit à dix lignes*),

[129] "Baudelocque regarda ce renversement comme un vice de conformation de l'organe."—(*Ibid*, p. 39.)

[130] *Clinique Chir.*, iii., 379.

[131] "Elle est posible même avant la grossesse, ainsi que Diemerbroeck, (*Anat. du Corps humaine*, i., 306,) le prouve déjà, et que j'en ai observé un cas." (Velpeau, *Traité des Accouchemens*, ii., 618.) I have sought in vain for Diemerbroeck's case, as here quoted, in the Latin edition of his works. Are we to add this to the numerous errors in the quotations and references by Velpeau, and regard them as the result of vicarious authorship in the literary department?

[132] *Traité des Mal. Chir.*, x., 489

of a pyriform shape, and rather larger than the uterus in its normal state; its pedicle was large, but short, and was encircled by a thin ridge (*bourrelet peu saillant*), within which the tip of a finger or a probe could pass to the depth only of a few lines. The surface of this tumour was unequal, sensible to the touch, of a greyish colour, and villous appearance; there were superficial ulcerations, which healed and re-appeared successively. At each menstrual period the sanguineous fluid could be seen to exude from the surface of the tumour, in drops separate from each other, which soon, by increase of number as well as of size, united together, forming a covering to the whole. In the intervals between the menses, the same surface furnished a yellow mucus. The tumour could readily be pressed up within the labia, but descended as soon as the pressure was discontinued. There rests no doubt of this being an example of inversion of the womb, in the chronic stage; but had there not been a concealed pregnancy of recent date, some months or a year or two, which gave rise to it? Boyer seems to have been satisfied that no such cause had preceded. The patient would readily have submitted to the ligature for removal of the disease, but Boyer feared that the effects of such treatment would be fatal, and advised her to continue with her present inconvenience, more especially as she enjoyed good health.

I anticipate that the cautious and enlightened reader will regard few of the histories contained in this section indisputable, besides the occurrence of inversion from the hydatidous intra-uterine tumour; but it seemed scarcely allowable, in an Essay intended to be systematic, to pass over statements proceeding from such high authorities as those I have just enumerated. Thus far it has been my endeavour chiefly to present undoubted facts, devoid of theories. In the remaining part of this Essay there will be ample opportunities for reasoning upon these facts, in considering the *causes, diagnosis,* and *treatment* of uterine inversion, including the *removal of the organ;* and until each succeeding section is actually written and in type, I shall assiduously employ myself in collecting further information, and thankfully receive it, through any of the numerous channels which have been so promptly and so liberally opened to me, not only in the United Kingdom, but in various parts of Europe, and in far more distant countries.

Plate 1.

C. Short del. Norwich.

J. Perry lithog.

Hullmandel & Walton Printers.

EXPLANATION OF THE PLATES.

PLATE I.

This plate refers to the case, in which I removed by ligature the completely inverted and prolapsed uterus, one month after delivery; it is of the natural size. The pyriform shape of the organ, and the nodulated surface, are observable. In order to show the thickening (*a*), which indicates the precise situation of the cervix, the mass has been pulled downwards a little; when left to itself, the cervix was just concealed within the external labia. The case is narrated fully in the *Provincial Medical and Surgical Journal* of June 12, 1844, p. 155; and in Braithwaite's *Retrospect of Practical Medicine and Surgery*, vol. x., p. 256.

In her first labour the patient had a difficult time, and was delivered with the forceps, for which purpose I was summoned by the accoucheur in attendance. Half an hour afterwards there was uterine contraction; and the placenta descending so as to be felt in the vagina, with my hand I brought it past to the *os externum;* but although nine-tenths of the placenta were thus made visible, a portion still remained adherent to a mass occupying the vagina, as if there were another placenta. I succeeded in detaching the placenta on a level with the external labia, avoiding further traction, and then grasping the mass in the vagina, I carried it up into the uterus. All these occurrences occupied very little time; the patient was losing blood, had become pallid and faint, and promptitude of action was required. Reflection confirmed my belief that the uterus had been inverted till it occupied the vagina; and in the relaxed state of the organ, I had readily *reverted* and placed all *in situ*. There were unfavourable symptoms for a few hours; the abdomen became large and tympanitic; but she recovered. Fifteen months afterwards, when she had arrived at the full term of her second pregnancy, and nearly completed her thirty-first year, she was delivered of a healthy child, under the superintendance of a different surgeon, who found profuse hæmorrhage, and deemed it necessary to detach the placenta, which he removed piece-meal. The hæmorrhage abated, but the patient continued faint, pallid, cold, and with a pulse scarcely to be felt, for four or five hours. She then rallied; and at the expiration of thirty hours the catheter was required, on account of retention of urine, when I discovered in the vagina a round body, as large as my fist, which I recognised to be the uterus totally inverted. Having requested the attendance of the surgeon who had officiated at the delivery, I drew off three pints of urine, and proceeded to replace the uterus, trying the various manipulations advised on such occasions. All the efforts both of my

associate and myself proved unavailing; the uterine tumour was firm and contracted; no advancement towards reduction was effected after an hours' trial, and we deemed it prudent to desist, the patient's pulse being 120, the abdomen distended, and the symptoms indicating immediate danger. At the expiration of four days, under an effort at defæcation, the inverted uterus prolapsed at the labia. I supported the tumour with bandages, to protect it from the external air, and to diminish its size, which, after one month had elapsed, answered to the representation here given. The termination of the vaginal membrane in the uterine membrane, answering to the os, was very distinguishable, as well as the circular thickening answering to the cervix. The particulars of the successful removal of the entire organ by ligature will be subsequently noticed.

PLATE II.

The drawing in this case is made from a preparation in the possession of Dr. Joseph Warrington, of Philadelphia, and was forwarded to me by my associate and most valued correspondent, Dr. C. D. Meigs, Professor of Midwifery, who vouched for the correctness and fidelity of the representation. The plate is half the size of the original, which was obtained on autopsy of a female, who died speedily after delivery at the full term of pregnancy, from *foudroyante* hæmorrhage, in the hands of her accoucheur, before the consultant arrived. It exhibits an anterior view of partial inversion in the greatest degree, (*perversio*,) the cervix uteri not being inverted. The whole of the organ was still within the vagina at the time of the patient's decease, so that the nature of the case was only discovered on a digito-vaginal examination. The inverted fundus (*a*) was just within the external labia; and as death happened when the organ was in a state of collapse or inertia, the inverted body of the uterus (*b*) is observed to be flaccid, and to have fallen into longitudinal folds at its superior part, next the cervix; it is also flattened anteriorly, whilst the os tincæ (*c*) is relaxed and expanded to a great size. The vaginal membrane (*d d*) covers the uninverted cervix, and above is (*e*) the bladder.

PLATE III.

Is a posterior view of the same preparation in Dr. Joseph Warrington's possession, (*a*) again designating the fundus. The inverted body of the uterus is flattened in this aspect also, and is partly concealed by the peritoneal membrane (*b b b*), which covers the uninverted cervix (*c c*), and descends to line the pouch or cul-de-sac, answering in depth and circumference to the displaced portion of the uterus. The ovaria (*d d*) are just above the cervix, as are also (*e e*) the Fallopian tubes. The broad ligaments (*f f*) are stretched, and in part sustained above the ample entrance into the peritoneal pouch referred to by the dotted white line (*g*). The posterior part of the bladder (*h*) forms the summit of the figure.

Plate 9

W. McIlvaine del. Philadelphia

J. Perry lithog

Hullmandel & Walton Printers

Plate 3.

W. Mc Ilvaine del. Philadelphia.

J. Perry lithog.

Hallmandel & Walton, Printers

Plate 4.

Allan Burns del Glasgow

Perry lithog.

Hallmandel & Walton Printers

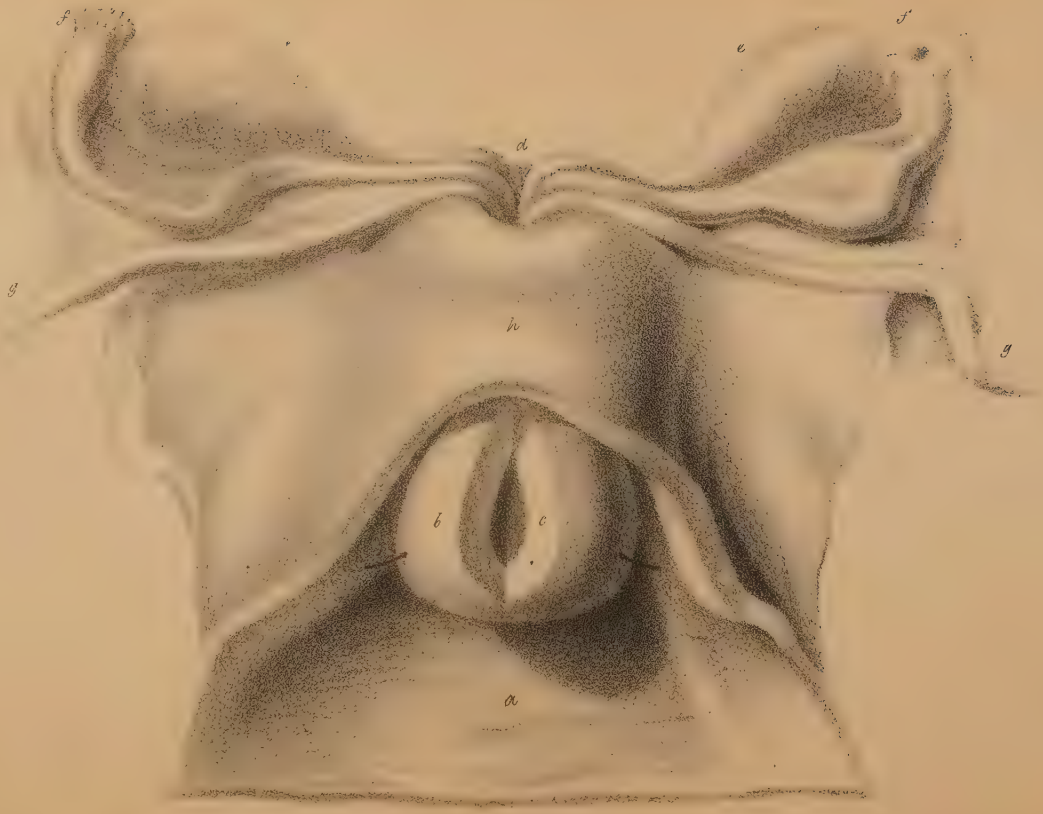

f
f
e
d
g
h
g
b
c
a

PLATE IV.

This plate is after a drawing executed for me by my friend, Dr. Allan Burns, of Glasgow, just before his decease. It represents a preparation in the Hunterian Museum in the University of that city, marked C. C. No. 60 *a*., at p. 143 of the printed catalogue of that Museum. A section is made in the median line to expose the interior, and the whole is spread out by the cut surfaces being separated from each other; this deranges the shape and apparent size of the parts, but allows of the interior being more distinctly observed and explained. The texture is like that of the healthy uterus, to which it also corresponds in size. The interior surface of the vagina (*a*) is blended with the os uteri, so that there is little distinction between them. The continuous cut surfaces of the vagina and uninverted portion of the uterus are marked (*b b*), the boundary being indicated by the greater width of the uterine surface, on which the left (*b*) rests, whilst the right (*b*) is upon the divided vagina. The inverted portion (*c c*), being the fundus and the greater part of the body of the uterus, is above an inch long. The angle of inflexion is defined by (*d d*). The dotted white line (*e*) occupies the peritoneal pouch, into which descend the Fallopian tubes (*f f*), the round ligaments (*g g*), and the broad ligaments (*h h*). The Fallopian tubes and the round ligaments of opposite sides approach each other at their termination at the fundus; (*i i*) are the ovaria. Some account of this preparation is given by Dr. John Burns, in foot-note ‡ p. 561 of his *Principles of Midwifery*, ninth edition. I consider that this preparation affords an example of partial inversion in the second degree, (*introversio*,) a portion of the body of the uterus, as well as its cervix and os, remaining uninverted.

PLATE V.

This is an original drawing, recently made by M. Biot, from the preparation now preserved in the *Musée Dupuytren* in Paris. Its dimensions are somewhat smaller than those of Segard's representation, in his *Dissertation sur les Polypes Utérins*, 1804, (plate 2, p. 31,) owing to the action of spirit upon the specimen during forty years. Dailliez, (*Thèse, Obs.* iv., p. 34,) gives an account of the case, which is still more fully narrated by Bayssellance, (*Diss. sur le Renversement de l'Utérus*, 1803, p. 25.) The patient was brought to l'Hospice de Perfectionnement in 1799, pallid, exhausted, and almost without a pulse, from continual losses, either serous or sanguineous, since her last delivery, being a period of twenty-two months. It was soon ascertained that an inversion of the womb was the cause of all the evil; but the malady was regarded as incurable, on account of its long standing and the exhausted condition of the patient, who expired just fifteen days from her admission into the hospital. The interior and upper part of the vagina is marked (*a*); the inverted fundus of the uterus (*b*) is seen to project beyond the dilated os; the inverted body of the organ has been cut through, and (*c*) rests upon one of the divided surfaces; thus the

peritoneal pouch, formed by the inversion, is laid open. Bristles are inserted into the terminating orifices of the Fallopian tubes. The opening from the abdomen into the peritoneal pouch, formed by the inversion, is indicated by (*d*), towards which both the Fallopian tubes (*f f*) and the round ligaments (*g g*) converge, till they meet and descend into the peritoneal pouch. The ovaria (*e e*) are nearer to each other than in the normal state, and are seen above the Fallopian tubes. The view offered is anterior, and the uninverted cervix uteri is covered and concealed at (*h*) by the peritoneum and parts beneath it.

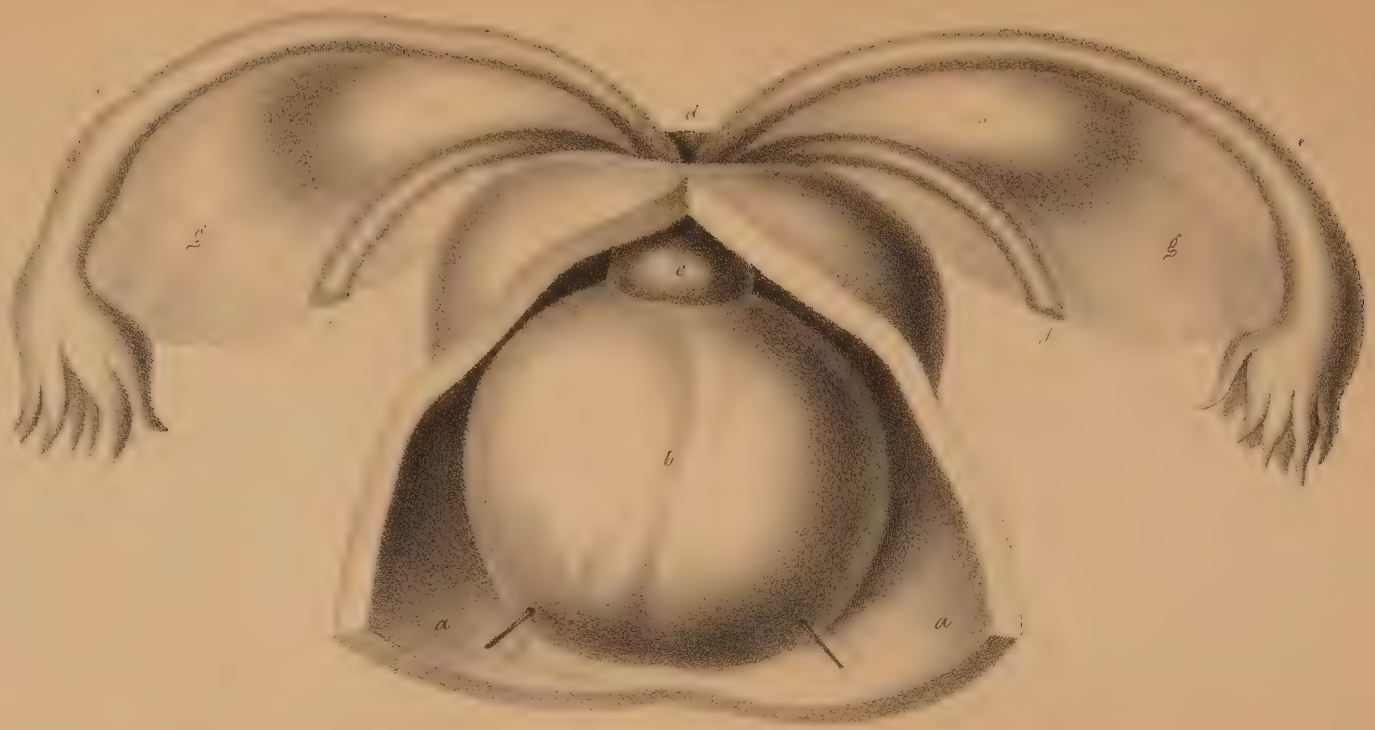
PLATE VI.

The preparation represented in this plate is preserved in the Museum of the Royal College of Surgeons of Ireland, and is noted in Dr. Houston's descriptive Catalogue of that Museum, vol. 2, p. 509, F. c. 691. There is no previous history of the individual to whom the preparation refers. Dr. Houston himself met with the specimen in the dissecting room, and remarks to me that the woman appeared to have borne children, but was not old; it was a case of chronic inversion. The dilated, but otherwise healthy vagina (*a*), contains the totally inverted uterus (*b*), of a globular shape, and with a smooth, unbroken surface; two bristles mark the terminating orifices of the Fallopian tubes upon the inverted fundus. The narrow pedicle (*c*) is continuous with the vagina above, and is hollow and lined with the peritoneum, giving passage to the Fallopian tubes and round ligaments, which converge until they meet at (*d*), where is clearly represented the circular entrance into the peritoneal cul-de-sac formed by the inversion. If we trace the Fallopian tubes (*e e*) from their fimbriated extremities till they meet at (*d*), and imagine their further course through the inverted organ to their terminating orifices, marked by the bristles, we cannot fail to notice their great length, as if stretched or elongated in consequence of the displacement. The visible portions of the round ligaments answer to (*f f*); those of the broad ligaments to (*g g*); and as an anterior aspect of the preparation is presented, the ovaria (*h h*) are distinguishable through the broad ligaments, and are approximated towards each other, so as to rest at the border (*d*) of the entrance into the peritoneal cul-de-sac before mentioned.

PLATE VII.

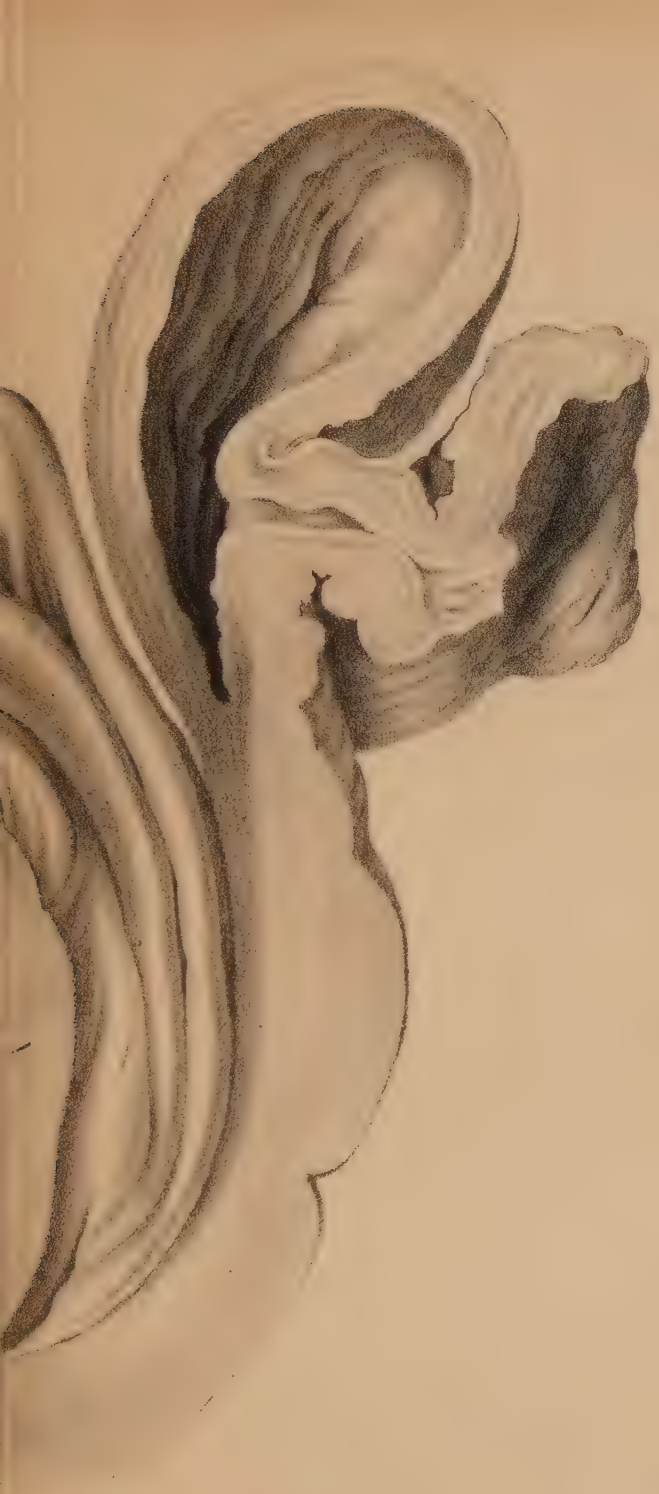
The original specimen is preserved in the Anatomical Museum of Queen's College, Birmingham, and was obtained in the consulting practice of Dr. Ingleby, whose decease, so suddenly premature, and so deeply lamented, has just occurred, and been to me a most painful incident associated with the composition of this much studied, yet hastily written Essay.

After the minute descriptions already given, I have deemed it preferable to avoid defacing the beautiful, and I may add, from my own observation, the faithful drawing



W Neilan del Dublin

J. Perry lith.



of the artist, Mr. Samuel Moreton, by *letters of reference*, conceiving that they might be dispensed with, as unnecessary to the instructed and attentive reader.

The patient, a woman of delicate constitution, continued weak after her first and only delivery, and had frequent menorrhagia; at length an examination was made, and it was decided that she had an uterine polypus; but the mistake was discovered just in time to prevent an operation being performed under that impression. Becoming more and more exhausted by hæmorrhage, the patient died nine months after her delivery.

The plate represents partial inversion, merging into the commencement of the third degree, (*perversio*,) the fundus just projecting, in a somewhat conical form beyond the os uteri, which may be seen in deep shade to the right, and more distinctly to the left, where the fundus is indented from attempts made to replace the uterus; all the rest of the inverted portion of the uterus is inclosed in the uninverted. The vagina is laid open by a longitudinal and anterior section; its rugæ and folds are spiritedly shaded. One round ligament is cut off shorter than the other; the longer, on the left half of the paper, is more distinctly to be seen. From each fimbriated extremity proceeds the slender Fallopian tube in elegant outline, and of great length; the two tubes are closely approximated where they disappear on entering the peritoneal cul-de-sac of the inversion. Between the round ligaments and the Fallopian tubes the observer will find the ovaria represented in glowing character; on one side, towards the right hand, are several cysts, connected with the left ovarium. In some respects the lithographer has heightened the effect which had been so ably produced by the original artist. Since the drawing was made the preparation has been diminished in size, by immersion for some time in *pure spirit*, instead of *diluted*, so that its present dimensions will be found less than those of the plate; but I can vouch for the plate corresponding in size, and in all other respects, to the preparation in its more recent state.

PLATE VIII.

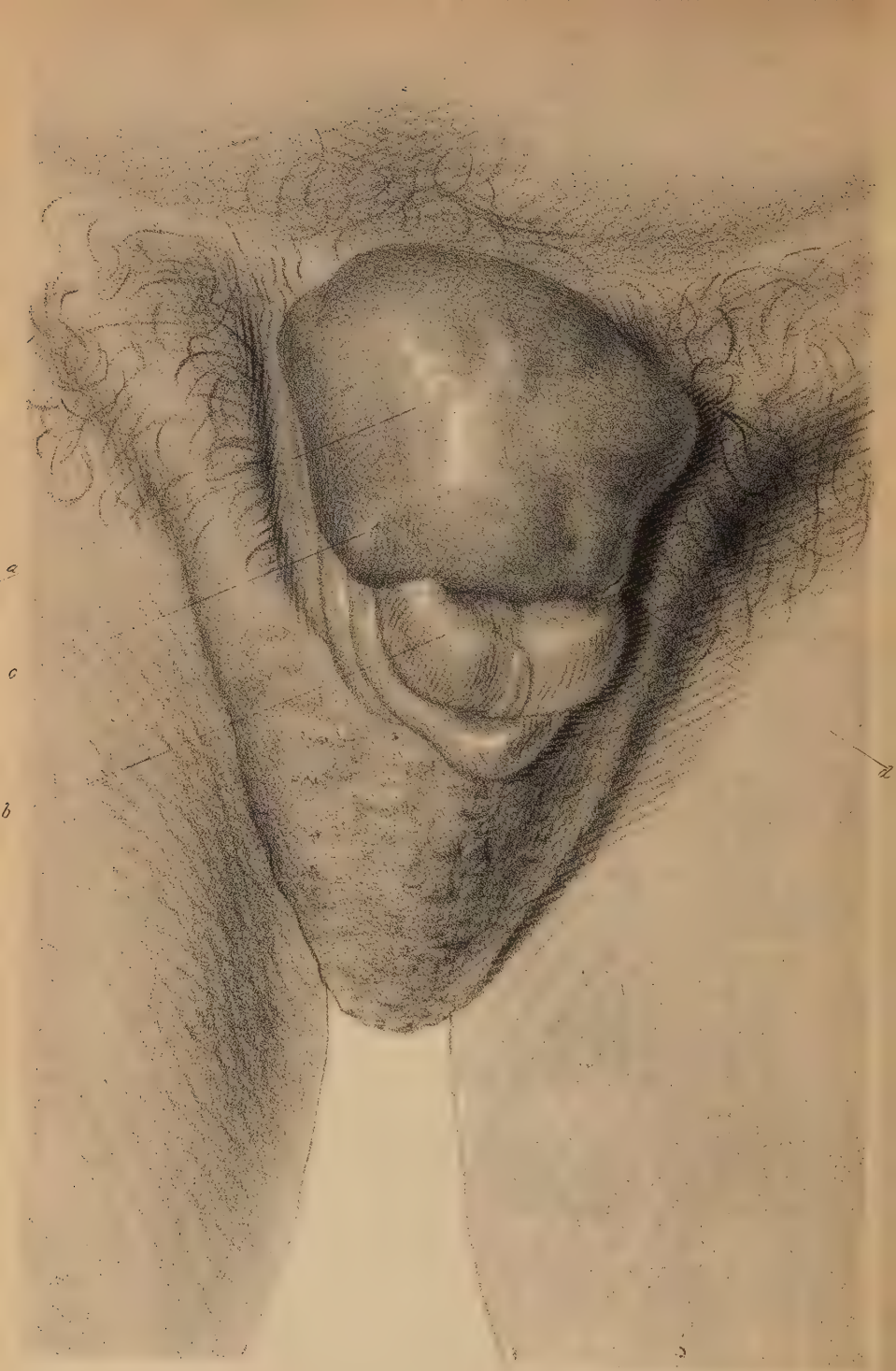
Through the liberality of the Council of the Royal College of Surgeons of Ireland, I obtained this representation of a most remarkable specimen of total inversion of both uterus and vagina, induced by the growth of a polypus from the internal fundus of the uterus. The case to which the plate has reference is briefly quoted towards the conclusion of section iv. of this Essay, and the preparation was met with at the Meath Hospital, in Dublin. By Dr. Houston's able method of displaying the morbid parts, the plate is rendered most clear and self-explanatory, and will be readily understood without any letters of reference. The left half of the preparation is exhibited in this plate, separated by a vertical section in the mesial line; the cut surfaces of the bladder, urethra, rectum, uterus, polypus, and vagina, are presented to view. At the inferior limit of the plate is the divided polypus, which was of the

size of a small pullet's egg, rough, and ulcerated on the surface, and attached by a rather narrow pedicle to the inverted fundus of the uterus. Next is the left half of the totally inverted uterus, equalling the natural size of that organ; its superior limit is indicated by an indentation or narrowing, being the termination of the uterine cervix and the commencement of the inverted vagina. The walls of the vagina are thickened, and its mucous membrane presents, in the specimen, several superficial patches of ulceration; in the intervals of the patches the covering is a white cuticular layer. The polypus, uterus, and great part of the vagina, formed a protruding mass at the external labia, between five and six inches in length. In the recent state, the polypous and uterine membranous surfaces were of a livid colour, and those of the vagina remarkably vascular. The bladder occupies the upper part of the drawing to the right; it is of moderate size, and its body and superior fundus rise up normally; but the inferior fundus is drawn down, so as to present, on the section, a conical cavity, nearly an inch in extent, with the apex downwards; all this displaced part of the inferior fundus of the bladder is below the level of the urethra, which is observable in its whole course, having been divided mesially by the vertical section.

Superiorly, and at the left boundary of the plate, is the half of the rectum, its cavity laid open, and its inferior termination characteristically marked; about two inches from the anal aperture, the section has very happily displayed a projection into the cavity of the rectum from its anterior wall, which Dr, Houston described as a valve of the rectum, several years ago, in the *Dublin Hospital Reports*, vol. v., p. 158, and which he considers the cause of the Rectum-Doctor being so often led upon the quicksands, when, in pursuit of his *ignis fatuus*, he thrusts a bougie through the coats of the bowel.

The parts which I have traced in the preceding description form together a boundary to the large peritoneal cul-de-sac, continuous with the general peritoneal cavity of the abdomen. This cul-de-sac is broadest superiorly, between the rectum and bladder, becomes narrower where it answers to the inverted vagina, and continues to diminish until it terminates conically at the inverted fundus of the uterus. In this cul-de-sac is observable the left ovarium, Fallopian tube, and round ligament; and it contained, also, when first examined, a coil of small intestine, not here represented, but which might have been included in a ligature, had this method of removing the disease been resorted to.

The preparation, marked F. c. 690, and described in the Catalogue of the Museum in which it is deposited, vol. ii., p. 508, is minutely injected. Dr. Houston, in the language of an enthusiastic anatomist, calls it "a speaking picture." It is certainly an unrivalled specimen of inverted displacement of the uterus and its appendages, induced by the presence of a small polypus.



a. Protruded Bladder. *b. rudimentary penis.*
c. opening of right ureter *d. opening of left ureter.*

T. Boisragon M.D. del. et lith.

Printed by G. Rowe, Cheltenham.

CASE OF

CONGENITAL MALFORMATION OF THE URINARY ORGANS.

BY HENRY GILES, ESQ.

Read at the Anniversary Meeting of the Provincial Medical and Surgical Association, held at Northampton, Thursday, August 8th, 1844.

Francis Hyde, the subject of the annexed drawing, was born in London, in the year 1822. He was removed from thence, when about fourteen, to Grimsthorp, near Bourne, in Lincolnshire, where he is still residing. I first had an opportunity of seeing him about two years since, through the kindness of Mr. Nicholls, an eminent surgeon in that neighbourhood, to whom I am also indebted for the great assistance he has afforded me in presenting the case to the Association.

Hyde is now about twenty-two years of age, in good health, and capable of considerable exertion; he is small, but not ill-formed, with the exception of the parts to be noticed, of the state of which the drawing gives an excellent idea. A tumour presents itself at the lower part of the abdomen, of a bright red appearance; this on examination is found to be the bladder, presenting its mucous surface without any covering or protection. It passes from its proper cavity beneath the symphysis pubis, and between the cruræ of the corpora cavernosa of the penis, previous to their union; after passing under the pubis it turns upwards and rather backwards, overlapping the symphysis and pressing against the centre of the lower part of the abdomen, which has become concave for its reception, giving the idea, at first sight, that it passed out above the symphysis. The protruded portion is generally about the size represented in the drawing; sometimes it is larger, at others smaller, contracting

so much that it could be covered with a shilling, and appearing as though it were likely to be completely withdrawn. I formed an idea that this state was produced voluntarily, but found him incapable of exercising the least controul over it. Externally and inferiorly, on both sides, the ureters open on the exposed surface, from which the urine is seen oozing, generally in drops; but if a little liquid be given him, it is thrown out in a small jet; this is also the case on his rising after laying down for a few minutes; very little forms during the night; it is at all times exceedingly acrid, and deposits large quantities of sand. Immediately under the protruded bladder, and to some extent supporting it, are the rudiments of a penis; it seems to be formed chiefly of the corpora cavernosa, united along their inferior and internal margins; superiorly they are turned outwards, and form a flat surface, on which, as before stated, the bladder rests: this is covered with a sort of fibrous membrane, which presents several openings of various sizes. There are no traces of either an urethra or glans penis. At the union of the corpora cavernosa anteriorly, is placed a species of frenum. The scrotum is well formed, and the testicles are fully developed. He occasionally experiences strong venereal excitement; the penis at these times becomes somewhat distended and elongated; the seminal fluid oozes between it and the bladder. From constant efforts to protect the parts in walking, the inferior extremities have become bent outwards at the knee, making his gait extremely awkward.

It should be mentioned, that at the time of his birth, only a small opening existed at the bottom of the body, through which the urine ran: this continued until he was six or eight months old, when a small tumour began to protrude, gradually increasing in size. His friends consulted Sir Astley Cooper and other distinguished members of the profession, who considered interference useless as well as dangerous.

A case, in some respects similar, is reported in the *Memoirs of the Academy of Sciences*, (Paris,) for the year 1761.

In conclusion I beg to express my obligation to Dr. Theodore Boisragon, of Cheltenham, for undertaking, and so ably executing, the accompanying drawing.

STOURBRIDGE,
August, 1844.

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Caleb Williams, Esq., Surgeon to the Retreat, and Lecturer on Materia Medica in the Medical School, York.

NON-RESIDENT.

John Francis Dale Yonge, M.D., Coblentz.

Peter Francis Luard, M.D., late of Warwick.

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Andrew Carrick, M.D., Clifton	1833–34
John Johnstone, M.D., F.R.S., Birmingham	1834–35
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Edward Holme, M.D., F.L.S., Manchester	1836–37
Henry C. Boisragon, M.D., Cheltenham	1837–38
Edward Barlow, M.D., Bath	1838–39
Thomas Jeffreys, M.D., Liverpool	1839–40
George Steed, M.D., Southampton	1840–41
George Goldie, M.D., York	1841–42
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William Hey, Esq., Leeds	1843–44
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Bell, David, M.D., Physician to the Dispensary, Ludlow, Salop.
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Carr, John, M.D., removed from West Cowes to 1, Russell Place, Fitzroy Square, London.
Crabbe, Alfred, Esq., Surgeon, Poole, to be erased from the list.
Deshon, Henry C., M.D., Budleigh Salterton, Devon.
Lampard, J., Esq., Surgeon, removed from Frome to Warminster, Wilts.
Morgan, John Brickdale, Esq., Surgeon, removed from Colton.
Riccard, Edward, M.D., South Molton, Devon.

ERRATUM.

Page 79, line 14. In part of the impression:—*for* "five ounces of half-boiling water," *read* "five ounces and a half of boiling water."

As it is scarcely possible to avoid errors in printing so numerous a list of Members, the Secretary is anxious, wherever they are committed, that they may be amended; and will therefore feel obliged if those Members whose designation and address are not rightly inserted, or whose Christian names are omitted, will send the requisite corrections.

THE FOLLOWING ARE

THE PRINCIPAL OBJECTS

TO WHICH THE ATTENTION OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION

IS DIRECTED.

1st.—Collection of useful information, whether speculative or practical, through Original Essays, or Reports of Provincial Hospitals, Infirmaries, or Dispensaries, or of private practice.

2nd.—Increase of knowledge of the Medical Topography of England, through statistical, meteorological, geological, and botanical inquiries.

3rd.—Investigations of the modifications of Endemic and Epidemic Diseases, in different situations, and at various periods, so as to trace, so far as the present imperfect state of the art will permit, their connections with peculiarities of soil or climate, or with the localities, habits, and occupations of the people.

4th.—Advancement of Medico-Legal Science, through succinct reports of whatever cases may occur in Provincial Courts of Judicature.

5th.—Maintenance of the honour and respectability of the Profession, generally, in the Provinces, by promoting friendly intercourse and free communication of its Members, and by establishing among them the harmony and good feeling which ought ever to characterise a liberal profession.

THE LAWS

OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

1st.—That a Provincial Medical and Surgical Association be formed.

2nd.—That the Association be managed by a President, a President of the Council, Secretary or Secretaries, and Council.

3rd.—That the several Officers be appointed annually, by a General Meeting of Members convened for that purpose, at whichever of the principal towns may be appointed; the place of such Meeting being prospectively notified each year.

4th.—That at this Meeting shall be presented a Report, prepared by the Secretary or Secretaries, of the general state of the Association, its proceedings, and pecuniary accounts; the Report to be afterwards printed, and a copy supplied to every Member.

5th.—That at this Meeting one of the Members shall be appointed to give, at the next Annual Meeting, an account of the state or progress of Medical Science during the last year; or an Oration on some subject connected with Medical Science; or a Biographical Memoir of some eminent cultivator of Medical Science, who may have resided in the Provinces.

PRESIDENT OF THE ASSOCIATION.

6th.—That the office of President be honorary, and conferred on some senior Physician or Surgeon of eminence, resident in any of the provincial towns comprised in the circle of the Association.

OFFICERS AND COUNCIL.

7th.—That the Secretary or Secretaries be resident in Worcester, the place of publication, the duties being to attend to the printing of the *Transactions*, and to correct the press; to be present at the meetings of the Council, and to keep the minutes thereof; to correspond with the Members of the Association; and to receive and submit to the Council all papers transmitted for publication.

8th.—That the Council consist of a President and —— Members, to be selected from the principal provincial towns. The Council, with whom must rest the chief responsibility of publication, to have full power of deciding on all papers transmitted; and the consent of three of its Members must be obtained before any paper can be published. It shall also be the duty of the Council to receive the subscriptions, when due, in their respective districts.

9th.—That the President of the Council be also the Treasurer of the Association, and keep the financial accounts.

REGULATIONS OF THE DISTRICT BRANCHES.

10th.—That Members of the Association be at liberty to form District Branches wherever it may suit their convenience.

11th.—That in order to facilitate the formation of such Branches, and maintain uniformity amongst them, the General Council provide suitable instructions for the guidance of those who may unite in instituting them.

12th.—That conformity with these instructions be further ensured, by the initiating proceedings and organization of each Branch being submitted to the General Council for their revision and approval.

13th.—That the District Branches be free to govern themselves as their respective Members may think fit; but that the by-laws ordaining the special government be submitted to the General Council previously to their taking effect, in order to guard against the possibility of any such by-laws contravening the fundamental laws of the Association.

14th.—That the District Branches be empowered to present for Election such of their Members as it may be deemed advantageous to the general and local interests of the Association to place on the General Council.

15th.—That the expenses incurred by the District Secretaries in conducting the proceedings of the District Branches be defrayed from the general fund, provided such expenses do not, in any instance, exceed one-seventh part of the guinea subscribed by each Member enrolled in the District Branch.

16.—That if any circumstance arise in the formation of District Branches which calls for a larger expenditure than what is allowed by the foregoing resolution, such expenses, provided they do not exceed one-fourth of the guinea, may be allowed by a statement of the circumstances being made known to the General Council.

ELECTION OF MEMBERS, ANNUAL MEETINGS, &c.

17th.—That each Member, on applying for admission, be nominated by two Members, as a pledge of eligibility.

18th.—That at each Annual Meeting the place of meeting for the ensuing year shall be announced.

19th.—That any Member wishing to propose a new law, or an alteration in an existing law, must send notice of his intention to the Secretary or Secretaries three months previous to the Anniversary Meeting, which will be circulated with the Report of the Council.

20th.—That the Association appoint Honorary Members at the Anniversary Meetings only.

21st.—That the Association appoint Members in foreign countries, to be styled "Honorary Corresponding Members," from whom communications, respecting the state of medicine in those countries, is expected to be received.

BENEVOLENT FUND.

22nd.—That a Medical Benevolent Society, under the restrictions proposed in the Report of the Committee presented to the Anniversary Meeting at Oxford, be connected with the Association.

PAYMENT OF SUBSCRIPTIONS, &c.

23rd.—Each Member of the Association to pay one guinea admission, and the same amount annually afterwards; the subscription to commence from the 1st of January each year, and to be considered as due, unless notice of its being withdrawn be given to the Secretary or Secretaries antecedently to the year for which it would be payable; for such subscription each Member shall receive a copy of each part of the *Transactions* published, which shall contain a list of all the Members, and be entitled also to a weekly copy of the *Provincial Medical and Surgical Journal.*

24th.—That those Members who have not an opportunity of paying their subscriptions to a Member of the Council resident in their district, are requested to pay it through the medium of their own bankers, to Messrs. Robarts and Co., London, for Messrs. Berwick and Co., Worcester, to the credit of the Association, or by a Post-office Order, sent to the President of the Council or the Secretary, which mode of remittance is specially pointed out, from being found the most convenient in conducting the business of the Society.

25th.—That all Papers and other communications be addressed to the Secretary or Secretaries, at Worcester, and forwarded carriage free.

26th.—That Messrs. Berwick and Co., Old Bank, Worcester, be the Bankers of the Association.

ROBERT J. N. STREETEN,
SECRETARY.

RULES

OF

THE BENEVOLENT FUND

OF THE

PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

Before specifying the Rules which are to direct the proceedings of this Society, it is proper to repeat, in this place, the main principles on which it is established. It is not intended either to be a Benefit Society, or an Assurance Club; but, strictly speaking, a benevolent or charitable institution, founded and promoted for the express purpose of assisting our professional brethren, when struggling under the pressure of disease or other calamities. It is likewise proposed, under circumstances of peculiar urgency and distress, to administer relief to the widow or family of a professional man, who may have been deprived of the support and protection of a husband or parent. While, therefore, the sole design of this Society is to hold out the hand of charity and benevolence to a suffering and afflicted brother, or his family, it will not countenance improvidence or idleness, or evil habits of any kind.

The principle above stated has received the sanction of three successive meetings of the "Provincial Medical and Surgical Association;" and the following resolutions were finally adopted at the anniversary, held at Manchester on the 20th and 21st of July, 1836:—

1. That a charitable fund be created by donations and subscriptions of members of the Association, to be called "The Benevolent Fund of the Provincial Medical and Surgical Association."

2. That contributions be received from all persons friendly to the objects of the Society, though belonging neither to the Association nor to the profession.

3. That the object of the fund be the relief of medical men* under severe and urgent distress, occasioned by sickness, accident, or any other calamity.

4. That any medical man labouring under such afflictions, be considered a fit object for the charity.

5. That the claims of contributors shall, as far as possible, have the preference; but that contributions to the fund give no claim of right to relief, the fund being one of pure charity, and that each case be judged according to the urgency of the distress.

6. That under circumstances of peculiar emergency, relief may be extended to the widows and orphans of medical men, it being understood that it is not the design of this fund to relieve medical men from the necessity of providing for their families by ordinary life insurances, and such other means as prudence dictates.

7. That the management of the fund be conducted by Committees of the contributing members, annually appointed; the Central Committee to be at Cheltenham, and Local Committees, subordinate to the Central, in each of the principal cities and towns—the Central Committee having power to appoint Local Committees wherever they may be required.

All communications to the Central Committee to be addressed to the Treasurer and Secretary, Dr. Conolly, Castleton House, near Cheltenham.

* The Society will not acknowledge any one as a fit object of relief who has not received a regular professional education, and is either a Doctor of Physic, or a Member of the College of Surgeons, or of the Apothecaries' Company.

NOTICES.

All Papers and other Communications to be addressed to the Secretary, Dr. Streeten, Worcester, and forwarded carriage-free.

Those Members who have not an opportunity of paying their Subscription, which becomes due on the 1st of January in each year, to a Member of the Council resident in their district, are requested to pay it, through the medium of their own Bankers, to Messrs. Robarts and Co., London, for Messrs. Berwick and Co., Worcester, on account of the "Provincial Medical and Surgical Association;" or they may remit the amount through the Post-office to either the President of the Council or the Secretary.

Gentlemen wishing to join the Association, are respectfully requested to apply to some Member of the Council in their immediate district; to Dr. Hastings, President of the Council, or to Dr. Streeten, Secretary, Worcester.

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